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AMERICAN PSYCHOLOGIST

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THE HETEROSEXUAL AFFECTIONAL SYSTEM IN MONKEYS¹

HARRY F. HARLOW

University of Wisconsin

THE inspiration for this address came from observational data obtained from seven guinea pigs—two males and three females in a colony and two females brought in temporarily. Observations were provided by my ten-year-old daughter Pamela. These observations were made with love and endearment, and the behavior observed was endearment and love. Furthermore, these observations were made at a level of objectivity difficult for an adult to attain in this field.

Male and female guinea pigs are very fond of each other. They stare blissfully into the limpid pink or ruby or midnight-blue pools of each other's eyes. They nuzzle and they cuddle and the end production is not characterized by rush or rape. After all, one does not have to hurry if there is no hurry to be had. This, Pamela has witnessed several times. A caged, virgin adult female was brought by a friend for mating. Twirp, Pamela's large, black, gentle male, was put into the cage with the new female. He purred, nuzzled her, brushed up against her, smelled and licked her, and gradually conquered the frightened animal. A half-hour later they were snuggled up next to each other, peaceful and content, and they lived in bliss for several weeks until another friend brought in her female and Twirp repeated his patient, gentle approach. Twirp has convinced me that some male guinea pigs, at least, are endowed with an innate sense of decency, and I am happy to say that this is the way most male monkeys behave. I presume that there are some men who have as deep a depth of dignity as guinea pigs.

The guest stands, unfortunately, ended peaceful coexistence in the colony. For many months the five adult guinea pigs had lived amiably in one large cage, with Twirp in command and the second male playing second fiddle. While Twirp was host to the visiting females, White Patch commanded the permanent harem. When Twirp was reintro-

duced to the colony cage, it took but ten seconds to discover that he would not be tolerated. White Patch bared his teeth and lunged at Twirp, and to save the males, a new cage was acquired.

This led to various divisions of the females and led Pamela to discover particular male guinea pigs like particular female guinea pigs, and they squeal piteously when separated, even when the female is so bulging with babies that she can offer the male nothing in terms of drive reduction. Particular female guinea pigs like particular male guinea pigs. Tastes seem fairly stable, for even after weeks of peaceful residence with the unfavored male, the female will still attempt to get to her favorite male, and after weeks of quiet residence with unfavored females, the male will still try to get to his favorite female.

The females, like the males, defend their rights. In the happy one-cage days two females were separated from the group to care for their litters. White Thrush, in an advanced stage of pregnancy, lived alone with the males. When Chirp was returned to the colony cage after three weeks of maternal chores, both males approached enthusiastically, making friendly gestures. But Hell hath no fury like a female guinea pig spurned, and White Thrush would not tolerate infidelity. She hissed at Chirp, and lunged, and as Chirp fled from the cage, White Thrush pursued, teeth bared. The males also pursued, clucking and purring in anticipation. The males won, and White Thrush sulked the rest of the day. Guinea pigs apparently have a well-developed heterosexual affectional system.

Sex behavior in the guinea pig has been intensively investigated, and there are exhaustive studies on what has been called the sex drive, but I know of no previous mention of or allusion to the guinea pig's heterosexual affectional system. No doubt this stems from the paradigm which has been established for research in this area.

In a typical experiment a male guinea pig and a female guinea pig in estrus are taken from their individual cages, dropped into a barren chamber, and observed for 15 minutes. In such a situation there is a high probability that something is going

¹ This research was supported by funds received from the Graduate School of the University of Wisconsin, from the Ford Foundation, and from Grant M-4528, National Institutes of Health.



FIG. 1. Initial response to female sexual-present posture. The male subsequently accepted the invitation.

to happen and that it will happen rapidly and repeatedly. The thing that happens will be reliable and valid, and all that one needs to do to score it is to count. It is my suggestion that from this time onward it be known as the "flesh count." Sometimes I wonder how men and women would behave if they were dropped naked into a barren chamber with full realization that they had only fifteen minutes to take advantage of the opportunities offered them. No doubt there would be individual differences, but we would obtain little information on the human heterosexual affectional system from such an experiment.

Sex is not an adventitious act. It is not here today and gone tomorrow. It starts with the cradle, and as a part of the human tragedy it wanes

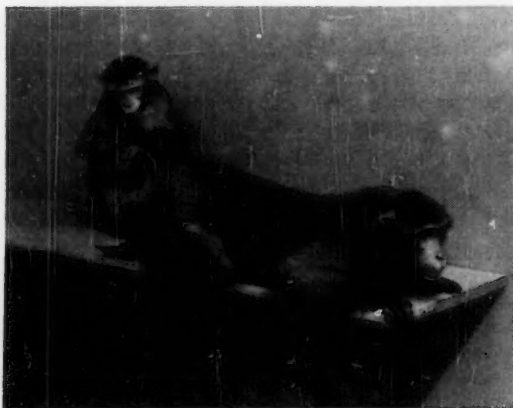


FIG. 2. Initial response to male sexual-present posture. The female (No. 48) subsequently approached and groomed the male.

before the grave. We have traced and are tracing the development of the heterosexual affectional system in monkeys.

We believe that the heterosexual affectional system in the rhesus monkey, like all the other affectional systems, goes through a series of developmental stages—an infantile heterosexual stage, a preadolescent stage, and an adolescent and mature heterosexual stage. Although these stages are in considerable part overlapping and cannot be sharply differentiated in time, we would think of the infantile stage as lasting throughout the first year and being characterized by inadequate and often inappropriate sexual play and posturing. The preadolescent stage, beginning in the second year



FIG. 3. Normal male and female sexual positioning.

and ending in the third year in the female and the fourth year in the male, is characterized by adequate and appropriate sexual play and posturing, but incompleteness. The adolescent and adult stage is characterized by behaviors which are similar in form but give rise to productive outcomes which are also reproductive.

Since in this paper sex is an unavoidable issue, we present illustrations of normal adult macaque monkey sex behavior. Sexual invitation may be initiated by the female, as in Figure 1, by a present pattern with buttocks oriented toward the male, tail elevated, and the female looking backward with a fear-grimace (not threat) pattern involving flattened ears and lip smacking. As you can see, this pattern need not involve rape nor even rush on the part of the male. The male may also solicit,

as in the case of the animal in the foreground of Figure 2; this animal has assumed a posture soliciting either grooming or more intimate favors. These patterns seldom elicit violent, uncontrolled, reflex behaviors. Normal male and female overt sex behavior is shown in Figure 3, the male having assumed the complex sex posture involving ankle clasp, dorsoventral mounting, and clasp of the female's buttocks. The partner demonstrates the complete female sexual pattern of elevating the buttocks, lowering the head, and looking backward. There have been millions of rhesus monkeys for millions of years, and there will be more in the future.

We have traced the development of the infantile heterosexual stage during the first year of life in two test situations using observational techniques. One is our playroom, illustrated in Figure 4, which consists of a room 8 ft. high with 36 feet of floor space. In this room are a platform, ladder, revolving wheel, and flying rings to encourage the infants' adaptation to a three-dimensional world, and there is an assortment of puzzles and toys for quieter activities. Two groups of four infants each, half of each group male and half female, have been observed in the playroom daily over many months. The second apparatus is shown in Figure 5. This is the playpen situation, and it consists of four large living cages and adjoining pens. Each living cage houses a mother and infant, and a three-inch by five-inch opening in the wall between cage and playpen units enables the infants to leave the home cage at any time but restrains the mothers. The playpen units are separated by wire-mesh panels which are removed one or two hours a day to allow the infants to interact in pairs during the first 180 days and both in pairs and in groups of four during the next half-year of life. Again, we are referring to data gathered from two playpen setups, each housing four infants and their real or surrogate mothers. Insofar as the infantile heterosexual stage is concerned, it makes little or no difference from which situation we take our data.

The outstanding finding in both the playroom and playpen is that male and female infants show differences in sex behavior from the second month of life onward. The males show earlier and more frequent sex behavior than do females, and there are differences in the patterns displayed by the sexes. The males almost never assume the female sex-posture patterns, even in the earliest months.

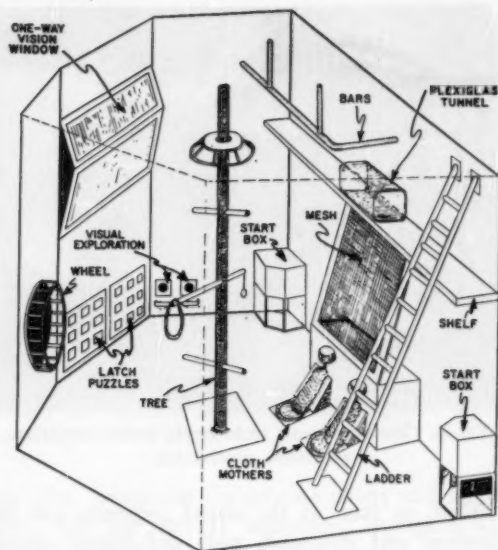


FIG. 4. Playroom test situation.

The females, on the other hand, sometimes display the male pattern of sex posturing, but this is infrequent after ten months of age. Predominantly, females show the female pattern and exceptional instances are to other females, not males. Frequency of sex behavior for both males and females increases progressively with age. There is no latency period—except when the monkeys are very tired.

The early infantile sexual behaviors are fragmentary, transient, and involve little more than passivity by the female and disoriented grasping and thrusting by the male. Thus, the male may thrust at the companion's head in a completely disoriented manner or laterally across the midline of the body, as in Figure 6. However, it is our opinion that these behaviors are more polymorphous than perverse.

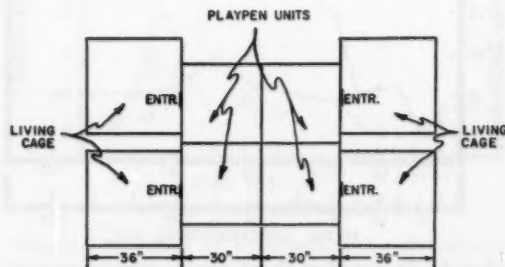


FIG. 5. Playpen test situation.



FIG. 6. Immature male and female sexual posturing, playroom observation.

Thus, as soon as the sexual responses can be observed and measured, male and female sexual behaviors differ in form. Furthermore, there are many other behaviors which differ between males and females as soon as they can be observed and measured. Figure 7 shows the development of threat responses by males and females in the playroom, and these differences are not only statistically significant, but they also have face validity. Analysis of this behavior shows that males threaten other males and females but that females are innately blessed with better manners; in particular, little girl monkeys do not threaten little boy monkeys.

The withdrawal pattern—retreat when confronted by another monkey—is graphed for the playroom in Figure 8, and the significance is obvious. Females evince a much higher incidence

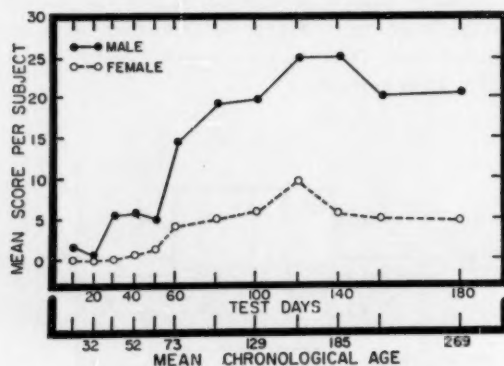


FIG. 7. Frequency of threat responses by males and females in the playroom.

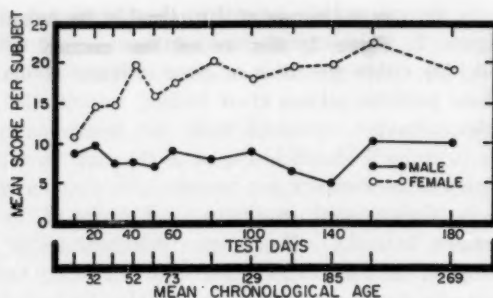


FIG. 8. Frequency of withdrawal responses by males and females in the playroom.

of passive responses, which are characterized by immobility with buttocks oriented toward the male and head averted, and a similar pattern, rigidity, in which the body is stiffened and fixed.

In all probability the withdrawal and passivity behavior of the female and the forceful behavior of the male gradually lead to the development of normal sex behaviors. The tendency for the female to orient away from the male and for the male to clasp and tussle at the female's buttocks predisposes the consorts to assume the proper positions. The development of the dorsally oriented male sex-behavior pattern as observed in the playroom situation is shown in Figure 9 and may be described as a composite yearning and learning curve.

Infant male and female monkeys show clear-cut differences in behavior of far greater social significance than neonatal and infantile sex responses.

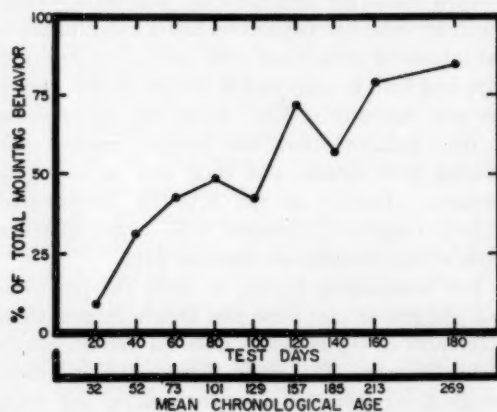


FIG. 9. Percentage of all male mounts (immature and mature) in the playroom that shows dorsal orientation (mature pattern).

Grooming patterns, which are basic to macaque socialization, show late maturation, but as is seen in Figure 10, when they appear, they sharply differentiate the two sexes. Caressing is both a property and prerogative of the females. Basic to normal macaque socialization is the infant-infant or peer-peer affectional system, and this arises out of and is dependent upon the play patterns which we have described elsewhere and only mention here. As is shown in the solid lines of Figure 11, play behavior in the playroom is typically initiated by males, seldom by females. However, let us not belittle the female, for they also serve who only stand and

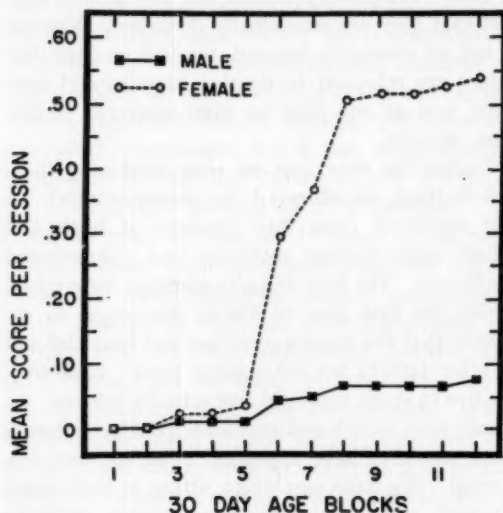


FIG. 10. Frequency of grooming responses made by males and females in the playroom.

wait. Contact play is far more frequent among the males than the females and is almost invariably initiated by the males. Playpen data graphed in Figure 12 show that real rough-and-tumble play is strictly for the boys.

I am convinced that these data have almost total generality to man. Several months ago I was present at a school picnic attended by 25 second-graders and their parents. While the parents sat and the girls stood around or skipped about hand in hand, 13 boys tackled and wrestled, chased and retreated. No little girl chased any little boy, but some little boys chased some little girls. Human beings have been here for two million years, and they'll probably be here two million more.

These secondary sex-behavior differences proba-

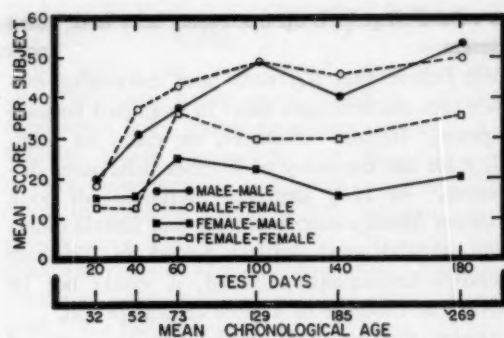


FIG. 11. Frequency of play-initiations by males and females to monkeys of the same (male-male, female-female) and other sex (male-female, female-male). Observations are from the playroom.

bly exist throughout the primate order, and, moreover, they are innately determined biological differences regardless of any cultural overlap. Because of their nature they tend automatically to produce sexual segregation during middle and later childhood, but fortunately this separation is neither complete nor permanent. Behavioral differences may very well make it easy through cultural means to impose a sexual latency period in the human being from childhood to puberty. We emphasize the fact that the latency period is not a biological stage in which primary sex behavior is suppressed,

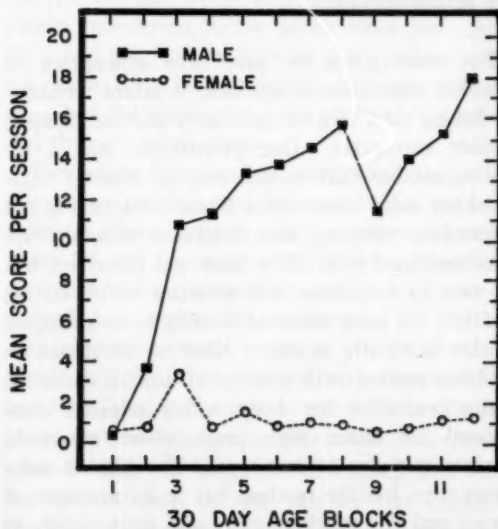


FIG. 12. Frequency of occurrence of "rough-and-tumble" play for two males and two females in the playroom through the first year of life.

but a cultural stage built upon secondary behavioral differences.

We believe that our data offer convincing evidence that sex behaviors differ in large part because of genetic factors. However, we claim no originality for the discovery of intersex behavioral differences. In 1759 Laurence Sterne in his book *Tristram Shandy* described male and female differences at the most critical period in Tristram Shandy's development; indeed, it would not be possible to conceive of a more critical period.

"Pray, my dear, quoth my mother, have you not forgot to wind up the clock?— Good G—! cried my father, making an exclamation, but taking care to moderate his voice at the same time— Did ever woman, since the creation of the world, interrupt a man with such a silly question?"²

Men and women have differed in the past and they will differ in the future.

It is possible that the listener has been dismayed by the frequent reference to sex and the relatively infrequent reference to affection. Out of these infantile behavior patterns, both sexual and non-sexual, develop the affectional bonds and the social ordering that appear to be important or even essential to the full development of the heterosexual affectional system of macaques. Traumatic affectional errors, both transient and prolonged, may have devastating effects upon subsequent social and sexual behaviors.

For some years we have been attempting to establish experimental neuroses in infant monkeys by having them live on unfriendly and inconsistent mother surrogates. One preparation was a rejecting mother that on schedule or demand separated her baby when a wire frame embedded in her spun-nylon covering was displaced violently upward and backward. The baby was disturbed, but as soon as the frame was returned to its resting position, the baby returned to cling to its surrogate mother as tightly as ever. Next we developed an air-blast mother with a series of nozzles down the entire center of her body which released compressed air under high pressure—an extremely noxious stimulus to monkeys. The blasted baby never even left the mother, but in its moments of agony and duress, clung more and more tightly to

the unworthy mother. Where else can a baby get protection? Apparently our infant had never read Neal Miller's theory that avoidance gradients are precipitous and approach gradients gradual and tenuous, for love conquered all.

We next devised a shaking mother, which on schedule or demand shook her infant with unconscionable violence until its teeth chattered. The infant endured its tribulations by clinging more and more tightly. At the present time we believe we may be on the threshold of success through Jay Mowbray's creation of the porcupine mother, which extrudes brass spikes all over its ventral surface. Preliminary studies on two infants suggest that they are emotionally disturbed. Whether or not we eventually succeed, the fact remains that babies are reluctant to develop experimental neuroses, and at one time we even wondered if this were possible.

During the time that we were producing these evil mothers, we observed the monkeys which we had separated from their mothers at birth and raised under various mothered and nonmothered conditions. The first 47 baby monkeys were raised during the first year of life in wire cages so arranged that the infants could see and hear and call to other infants but not contact them. Now they are five to seven years old and sexually mature. As month after month and year after year have passed, these monkeys have appeared to be less and less normal. We have seen them sitting in their cages strangely mute, staring fixedly into space, relatively indifferent to people and other monkeys. Some clutch their heads in both hands and rock back and forth—the autistic behavior pattern that we have seen in babies raised on wire surrogates. Others, when approached or even left alone, go into violent frenzies of rage, grasping and tearing at their legs with such fury that they sometimes require medical care.

Eventually we realized that we had a laboratory full of neurotic monkeys. We had failed to produce neurotic monkeys by thoughtful planning and creative research, but we had succeeded in producing neurotic monkeys through misadventure. To err is human.

Because of housing pressures some of these monkeys and many of our surrogate-raised monkeys lived in pairs for several years while growing to sexual maturity, but we have seldom seen normal sex behavior, and we certainly have not had the

² Sterne, Laurence. *The life and opinions of Tristram Shandy, Gentleman*. J. A. Work (Ed.), New York: The Odyssey Press, 1940, p. 5.

validating criterion of newborn baby monkeys. Instead, these monkeys treat each other like brother and sister, proving that two can live in complete propinquity with perfect propriety as long as no one cares.

Their reason for being, as we saw it, was to produce babies for our researches, and so at this point we deliberately initiated a breeding program which was frighteningly unsuccessful. When the older, wire-cage-raised males were paired with the females at the peak of estrus, the introduction led only to fighting, so violent and vicious that separation was essential to survival. In no case was there any indication of normal sex behavior. Frequently the females were the aggressors; even the normal praying mantis waits until the sex act is completed.

Pairing such cloth-surrogate-raised monkeys as were sexually mature gave little better end results. Violent aggression was not the rule, and there was attempted sex behavior, but it was unproductive since both the male and female behaviors were of the infantile type we have already described.

At this point we took the 17 oldest of our cage-raised animals, females showing consistent estrous cycles and males obviously mature, and engaged in an intensive re-education program, pairing the females with our most experienced, patient, and gentle males, and the males with our most eager, amiable, and successful breeding females. When the laboratory-bred females were smaller than the sophisticated males, the girls would back away and sit down facing the males, looking appealingly at these would-be consorts. Their hearts were in the right place, but nothing else was. When the females were larger than the males, we can only hope that they misunderstood the males' intentions, for after a brief period of courtship, they would attack and maul the ill-fated male. Females show no respect for a male they can dominate.

The training program for the males was equally unsatisfactory. They approached the females with a blind enthusiasm, but it was a misdirected enthusiasm. Frequently the males would grasp the females by the side of the body and thrust laterally, leaving them working at cross purposes with reality. Even the most persistent attempts by these females to set the boys straight came to naught. Finally, these females either stared at the males with complete contempt or attacked them in utter frustration. It became obvious that they, like their human counterpart, prefer maturer men. We realized then that

we had established, not a program of breeding, but a program of brooding.

We had in fact been warned. Our first seven laboratory-born babies were raised in individual cages while being trained on a learning test battery. William Mason planned to test their social behaviors subsequently, and great care had been taken to keep the babies socially isolated and to prevent any physical contacts. Neonatal baby monkeys require 24-hour-a-day care, and infant monkeys need ministrations beyond a 40-hour week. We had assigned the evening care to Kathy, a maternal bit of fluff who had worked for several years as a monkey tester while studying to become an elementary school teacher.

Checking on his wards one night near 10 P.M., Mason found Kathy sitting on the floor surrounded by seven baby monkeys, all eight of the primates playing happily together. Before the horrified scientist could express his outrage, Kathy had risen to her full height of five feet two. Already anticipating the carping criticisms which he was formulating, she shook her finger in his face and spoke with conviction: "Dr. Mason, I'm an education student and I know that it is improper and immoral to blight the social development of little children. I am right and you are wrong!"

Although we were angry with Kathy, we did think there was a certain humor in the situation and we did not worry about our monkeys. We simply transferred Kathy to an office job. Alas, she could not have been more right and we could not have been more wrong! We have already described the social-sexual life of these 7 monkeys and the next 40 to come.

Two years later we had more than theoretical reasons to be disturbed because Mason tested a group of these isolation-raised monkeys, then between 2.5 and 3.5 years of age, and found evidence of severe social abnormalities, which might be described as a sociopathic syndrome. He matched the laboratory-raised monkeys on the basis of weight and dentition patterns with monkeys that had been born and raised in the wild for the first 12 to 18 months, then captured and subjected to various kinds of housing and caging treatments for the next year or two. In the test situations the laboratory-raised monkeys, as compared with feral monkeys, showed infantile sexual behavior, absence of grooming, exaggerated aggression, and



FIG. 13. Group of cloth-surrogate-raised monkeys on the monkey island in the Madison Zoo.

absence of affectional interaction as measured by cooperation.

We are now quite certain that this sociopathic syndrome does not stem from the fact that the baby monkeys were raised in the laboratory but from *how* they were raised in the laboratory. Our infants raised in the laboratory by real monkey mothers and permitted opportunity for the development of normal infant-infant affection demonstrate normal male and female sexual behavior when they enter the second year of life. Furthermore, our playroom and playpen studies show that infant monkeys raised on cloth mothers but given the opportunity to form normal infant-infant affectional patterns, also develop normal sexual responses.

In a desperate attempt to assist a group of 18 three- to four-year-old cloth-surrogate-raised monkeys, half of them males and half females, we engaged in a group-psychotherapy program, placing these animals for two months on the monkey island in the Madison Zoo, as shown in Figure 13. Their summer vacation on the enchanted island was not without avail, and social grooming responses rapidly developed and were frequent in occurrence. After a few days of misunderstanding, patterns of social ordering developed, and a number of males and females developed friendship patterns. Unfortunately, sexual behavior was infrequent, and the behavior that was observed was completely inadequate—at least from our point of view. In desperation we finally introduced our most experienced, most patient, and most kindly

breeding male, Smiley (the male in Figures 1 and 2), and he rapidly established himself as king of the island and prepared to take full advantage of the wealth of opportunity which surrounded him. Fortunately, the traumatic experiences he encountered with unreceptive females have left no apparent permanent emotional scars, and now that he has been returned to our laboratory breeding colony, he is again making an important contribution to our research program. If normal sexual behavior occurred, no member of our observational team ever saw it, and had a female become pregnant, we would have believed in parthenogenesis.

But let us return to the monkeys that we left on the island and the older ones that we left in their cages. A year has passed, and the frustrations that both we and our monkeys experienced are in some small part nothing but a memory. We constructed larger and more comfortable breeding cages, and we designed a very large experimental breeding room 8 feet by 8 feet by 8 feet in size with appropriate platforms and a six-foot tree. Apparently we designed successful seraglios for I



FIG. 14. Typical behavior of unmothered mother toward her infant. Mother is looking upward while crushing her baby against the cage floor.

can report that not all love's labors have been lost. It does appear that the males are completely expendable unless they can be used in a program of artificial insemination. Certainly we can find no evidence that there is a destiny that shapes their ends unless some Skinnerite can help us with the shaping process. We have, however, had better success with some of the females, particularly the females raised on cloth surrogates.

Even so, one of the wire-cage-raised females is a mother and another is pregnant. Three cloth-surrogate females are mothers and four or five are expectant. We give all the credit to three breeding males. One, Smiley, does not take "no" for an answer. Smiley has a way with females. Patient, gentle, and persuasive, he has overcome more than one planned program of passive resistance. One female did not become pregnant until the fifth successive month of training. Month after month she has changed, and now she is mad about the boy. Male No. 342 behaves very much like Smiley. Even

when females threaten him, he does not harm them. Given time, he has been able to overcome more than one reluctant dragon, and he is a master of the power of positive suggestion.

Breeding male No. 496 has helped us greatly, particularly with the younger, cloth-surrogate-raised females. His approach differs from that of Smiley and No. 342. His technique transcends seduction, and in contract bridge terms it may be described as an approach-forcing system.

Combining our human and male-monkey talents, we are winning the good fight and imparting to naive and even resistant female monkeys the priceless gift of motherhood. Possibly it is a Pyrrhic victory. As every scientist knows, the solution of one scientific problem inevitably leads to another, and this is our fate (Figure 14). Month after month female monkeys that never knew a real mother, themselves become mothers—helpless, hopeless, heartless mothers devoid, or almost devoid, of any maternal feeling.

STUDIES ON THE GENERALITY OF AFFECTIVE MEANING SYSTEMS¹

CHARLES E. OSGOOD

University of Illinois

FOR the past ten years or so a group of us at the University of Illinois has been working on the theory and measurement of meaning. The theory is a two-stage, mediation conception of learning and the measurement technique is what has come to be called "the semantic differential." The early phases of this research, which are reported most completely in *The Measurement of Meaning* by Suci, Tannenbaum, and myself (1957), offered evidence for a reasonably stable and reproducible set of dimensions within which meaningful judgments are made. In recent years the major share of our efforts has been devoted to testing the generality of this semantic system. But before detailing this more recent work let me provide a context by briefly touching base with the learning theory and measurement models from which this research has come.

LEARNING THEORY AND MEASUREMENT MODELS

The upper portion of Figure 1 gives the paradigm considered essential for the formation of symbolic, meaningful processes. Significates (\hat{S}), such as the object APPLE, are assumed to elicit a complex pattern of total behavior (R_T), including automatic as well as skeletal reactions. When some other stimulus, as a potential sign (\hat{S}), such as visual perception of the object APPLE or auditory perception of the word "apple," accompanies or antedates the significate, it is assumed that this new stimulus becomes conditioned to some distinctive portion of the total object reaction, this portion coming to function in behavior as a *representational mediation process* (r_m). This process is representational because it is part of the very same behavior that the thing signified produces, hence its symbolic, semantic property; it is a mediation process by virtue of the fact that the self-stimulation it produces (s_m) can become associated with a variety of overt, adaptive acts (R_X) which "take account of" the thing signified. Psychological meaning is identified with such representational

mediation processes, and linguistic responses, as a subset of R_X , would seem to offer the most elaborate and discriminative possibilities for measuring meaning. However, in order to attain comparability and objectivity, it is necessary to devise a representative sample of scaled linguistic responses.

Now let me ask you to do the impossible—to imagine a space of some unknown number of dimensions; the center portion of this figure will help through at least three. This will be our hypothetical *semantic space*, and we may explore it by analogy with the more familiar color space. Like all self-respecting spaces, this one has an origin, which we define as complete "meaninglessness" (cf., the neutral grey of the color space). If we locate a sign as a point in this space, then its meaning could be represented by a vector from the origin to that point: the length of the vector would index the "meaningfulness" of the sign (cf., saturation in the color space) and its direction would index the "semantic quality" of the sign (cf., hue and brightness in the color space). Furthermore, distance between the endpoints of any two vectors in this semantic space should index the "meaningful similarity" of the signs thus represented. The central and lower portions of this figure suggest a coordination between learning theory and measurement models—namely, that vector direction in the measurement space is coordinate with which alternatives among a set of bipolar representational mediators are elicited by the sign and vector length is coordinate with how intensely these reactions are elicited.

But to talk about "direction" in any space we need some reference coordinates. One more analogy with the color space will prove useful to us: Just as complementary colors are defined as points equidistant and in opposite directions from the origin in the color space, which when mixed together in equal proportions cancel each other out to neutral grey, so may we conceive of *verbal opposites* as defining straight lines through the origin of the semantic space and canceling each other out to meaninglessness when "mixed." Imagine now a whole set of different straight-line

¹ Based on a paper presented to the American Psychological Association, September 3, 1961.

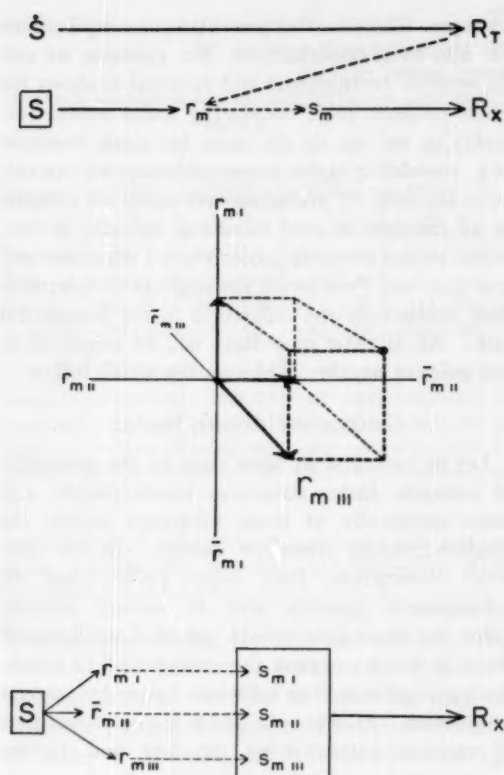


FIG. 1. The mediation Model (A) and its suggested coordination with the measurement model (B and C).

"cuts" through the space, each defined by a pair of opposites: We might have a subject indicate his "meaning" of a concept by a series of binary decisions—it is *beautiful* (not ugly), *soft* (not hard), *quick* (not slow) and so on. If these cuts were orthogonal, and hence independent of each other—a whopping big assumption that demands justification, of course—then each binary decision would reduce uncertainty about the location of the concept by half. Or, if each straight-line cut were scaled into seven discriminable steps, as we have done in our work, then each decision would reduce uncertainty of location by 6/7ths, and only three orthogonal cuts would yield a space of 343 regions.

But we still have the problem of reference coordinates. Is the up-down, north-south, and east-west of the semantic space to be completely arbitrary, or is there some "natural," built-in structuring of the space analogous to the gravitational and magnetic determinants of geophysical space? This question is an empirical one, and the logical tool

is some variant of factor analysis. We need to take a large and representative sample of dimensions defined by verbal opposites, determine their intercorrelations when used in judging the meanings of a representative sample of concepts, and then see if they fall into "natural" clusters or factors that can serve as reference coordinates. And since it is only through replication in the same domain that factor analysis becomes a hypothesis-testing device, we need to make repeated analyses using different subjects, concepts, and methods, being particularly careful that the rules for successive samplings of scales are independent of the factor structures found in previous analyses. Table 1 presents the results of three out of some 10 or more general factor analyses we have made on American subjects during the past decade. The scales shown for the three factors in order of magnitude under each analysis are the seven having the highest loadings, and they are typical of all the analyses we have done using either a broad sample of concepts or no concepts at all. An *evaluative factor*, which we identify as the attitudinal component of meaning, is characterized by scales like *good-bad*, *pleasant-unpleasant*, and *positive-negative*; what we call a *potency factor*,

TABLE 1
REPRODUCIBILITY OF THREE MAJOR FACTORS

Frequency-of-Usage Sampling Graphic Method (rotated) 20 Concepts/ 50 Scales	Frequency-of-Usage Sampling Forced-Choice Method No Concepts/ Same Scales	Logically Exhaustive Sampling Graphic Method (standard) 20 Concepts/76 (300) Scales
good—bad nice—awful beautiful—ugly honest—dishonest fragrant—foul fair—unfair sweet—sour	delicate—rugged nice—awful clean—dirty pleasant— unpleasant fragrant—foul smooth—rough good—bad	good—bad harmonious—dissonant successful—unsuccessful beautiful—ugly wise—foolish positive—negative kind—cruel
strong—weak large—small heavy—light rugged—delicate hard—soft bass—treble deep—shallow	strong—weak large—small heavy—light active—passive brave—cowardly thick—thin deep—shallow	hard—soft masculine—feminine severe—lenient strong—weak tenacious—yielding heavy—light serious—humorous
fast—slow active—passive sharp—dull hot—cold angular—rounded ferocious—peaceful tense—relaxed	sharp—dull fast—slow treble—bass active—passive bright—dark young—old high—low	fast—slow active—passive excitable—calm rash—cautious heretical—orthodox competitive— cooperative ornate—plain

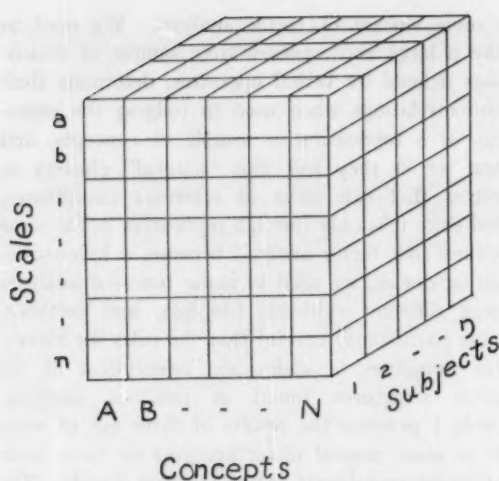


FIG. 2. The cube of data generated when η subjects judge N concepts against n scales.

orthogonal to evaluation, is characterized by scales like *strong-weak*, *heavy-light*, and *hard-soft*; what we call the *activity factor*, independent of both evaluation and potency, is characterized by scales like *fast-slow*, *active-passive*, and *excitable-calm*.

The problem for the present paper concerns the *generality of this semantic factor system*, both across the people doing the judging and across the concepts being judged. Is the evaluation-potency-activity system merely the lowest common denominator that averages out of many diverse individual systems or is it shared? Is it limited to Americans speaking the English language, or is it shared by all humans regardless of language and culture? Is there interaction between the semantic scales and the particular concepts being judged, such that we need different "semantic differentials" for different concept classes? To anticipate our general conclusions a bit, we will find that this factor system does seem quite stable across people but quite unstable across concept classes, and I will want to offer some speculations about the "whys" of this result.

However, before presenting the relevant studies, a methodological note is in order: As indicated in Figure 2, our kind of data represents a *three-way correlational and factorial problem* rather than the more usual two-way problem of subjects-by-tests. That is to say, when η subjects judge N concepts against n scales, a cube of data is generated within which there are three potentially independent sources of variation in factor structure—scales, subjects, and

concepts. This situation presents some complications but also some opportunities. For example, we can do separate correlational and factorial analyses for single subjects (e.g., correlating scales across concepts) or we can do the same for single concepts (e.g., correlating scales across subjects); we can collapse the cube by averaging over either all subjects or all concepts or over subsets of subjects or concepts; we can correlate subjects over both scales and concepts, and then factor the subjects to determine their similarities and differences in the judgmental task. All of these procedures will be employed at one point or another in the studies which follow.

GENERALITY ACROSS PEOPLE

Let us look first at some data on the generality of semantic factor structures across people, and more specifically at some subgroups *within the English-speaking American culture*. In her doctoral dissertation, Joan Bopp (1955) had 40 schizophrenic patients and 40 normal controls judge the same concepts (a set of Kent-Rosanoff stimulus words) against the same set of 13 scales. Both groups were then split into halves by random assignment. As shown in Table 2, the proportions of variance extracted by the first four factors

TABLE 2

PROPORTIONS OF TOTAL VARIANCE AND INDICES OF FACTORIAL SIMILARITY FOR FIRST FOUR FACTORS IN ORDER OF MAGNITUDE (UNROTATED) FOR CONTROL (A AND B) AND SCHIZOPHRENIC (C AND D) SUBGROUPS

Factors	Proportions of Total Variance					
	Control Groups (in per cent)		Experimental Groups (in per cent)			
	A	B	C	D		
1	61	63	59	64		
2	22	21	20	21		
3	7	6	7	5		
4	5	4	6	6		
Factors	Indices of Factorial Similarity					
	Normals	Schizophrenics	Normals vs. Schizophrenics			
	A with B	C with D	A with C	A with D	B with C	B with D
1	.95	.93	.98	.95	.98	.85
2	.95	.92	.96	.95	.95	.83
3	.88	.93	.73	.68	.91	.79
4	.87	.97	.77	.79	.94	.94

TABLE 3
ROTATED FACTOR LOADINGS (PRE-TEST/POST-TEST) FOR JUDGMENTS
OF INTERNATIONAL RELATIONS CONCEPTS

	Factor I			Factor II			Factor III			
	Experimental Course A	Traditional Course B	No Course C	Experimental Course A	Traditional Course B	No Course C	Experimental Course A	Traditional Course B	No Course C	
good	+97/+91	+97/+94	+94/+96	-00/-03	-16/-07	+10/+08	-17/+40	-00/-26	-22/-19	bad
kind	+96/+89	+93/+90	+93/+96	-13/-19	-32/-20	-01/-11	-24/+37	-08/-36	-33/-23	cruel
honest	+96/+93	+91/+91	+95/+97	-11/-11	-27/-19	+03/-10	-12/+27	+12/-20	-20/-13	dishonest
valuable	+92/+84	+92/+87	+93/+80	+06/+03	-13/-02	+05/+05	-26/+42	-05/-24	-29/-47	worthless
strong	+17/+06	-01/-08	+12/+07	+95/+91	+97/+97	+93/+97	-05/+33	-12/-04	-20/-10	weak
severe	-79/-78	-70/-72	-88/-88	+58/+55	+70/+64	+40/+44	+15/-25	-01/+22	+19/+13	lenient
tenacious	-58/-69	-52/-60	-80/-72	+78/+70	+81/+78	+55/+67	+06/+01	-07/-03	+14/+05	yielding
hard	-73/-78	-68/-67	-83/-80	+63/+59	+70/+70	+46/+56	+16/-17	-03/+16	+21/+07	soft
active	-02/-00	-00/+05	-02/+02	+95/+90	+94/+93	+94/+97	+12/-26	+18/+16	+17/-00	passive
fast	-19/-33	-43/-36	-31/-37	+84/+79	+81/+83	+77/+74	+37/-35	+24/+34	+43/+49	slow
excitable	-56/-57	-79/-62	-49/-82	+37/+11	+13/+13	+17/+10	+67/+74	+49/+70	+77/+47	calm
rash	-70/-51	-87/-62	-75/-69	+10/+12	+13/+18	+01/-05	+62/+78	+37/+42	+60/+62	cautious

(evaluation, activity, potency, and a specific hot-cold factor in that order) were nearly identical for all four groups. And for at least the first two dominant factors, the indices of factorial similarity (Wrigley & Neuhaus, 1955) were about as high between schizophrenic and normal subgroups as within them. In a study on the effects of a new, experimental course on international relations, McClelland, Whitaker, and First² had one group take the new course, another a traditional course, and a third group no course at all. A form of semantic differential, consisting of 20 relevant concepts and 12 scales drawn from our previous work, was administered to all three groups both prior to and after the taking of the courses. As can be seen in Table 3, not only are the Varimax rotated factor loadings nearly identical for the three groups, A, B, and C, despite their differences in educational experience with respect to international relations, but there are essentially no differences between pre- and post-tests. This result testifies to the stability and reliability of semantic factor structures across groups of people despite differences in relevant educational experience. But note that here, as with the schizophrenic-normal comparison, the fact that the factorial system stays constant does *not* imply that these groups necessarily shared the same mean-

ings for specific concepts—in many cases they did not.

I mentioned earlier that the three-way nature of our data makes it possible to do separate factor analyses for individual subjects and then compare them. My colleague, Edward Ware, did just this in a study relating intelligence and sex to semantic factor structure (1958). His primary hypothesis was that more intelligent people would display greater diversity in their semantic spaces. Male and female high school students, for whom standard IQ test scores were available, were divided into three groups of about 35 each—IQs 73-99, 100-110, and 111-133. All subjects rated 31 varied but simple concepts (e.g., SEX, MOTHER, BABY, SNOW) against 20 typical semantic differential scales, and separate factor analyses were run for individual subjects. Ware obtained several diversity indices for each subject—including the average standard deviation on scales, % variance accounted for by the first factor, cumulative % variance accounted for by the first three factors, and the entropy statistic, *H*, across factors—but despite the fact that reliable and significant individual differences were demonstrated for each diversity measure, there was no relation to intelligence whatsoever! But one should never give up a good hypothesis without a fight, and I still think there ought to be a relation between intelligence

² C. McClelland, U. Whitaker, and R. First. Personal communication. 1960.

and completeness of utilization of the semantic space. Ware also was unable to demonstrate any differences in diversity between the sexes, and no consistent differences in qualitative factor structure were found either across sexes or IQ levels.

So far we have been considering differences between individuals and groups who share the same language and culture. The most critical test of the generality of affective meaning systems should be *between people differing widely in both language and culture*. We have already made a considerable number of specific comparisons across cultures, but not really satisfying the criteria of an ideal design; we are now in the middle of a three-year project sponsored by the Human

Ecology Fund, involving some dozen countries, in which we are trying to at least approximate an ideal design. I will organize my report of this series of cross-cultural studies in terms of an idealized design, fitting in particular studies where they seem to apply.

An ideal design might be as follows: (1) We would use a sample of countries representing several different language families and diverse cultures; (2) we would obtain representative samples of polar qualifiers (e.g., adjectival opposites in English) independently in each language-culture community; (3) we would determine the factorial structure among these qualifier dimensions, (a) when simply related to each other directly and

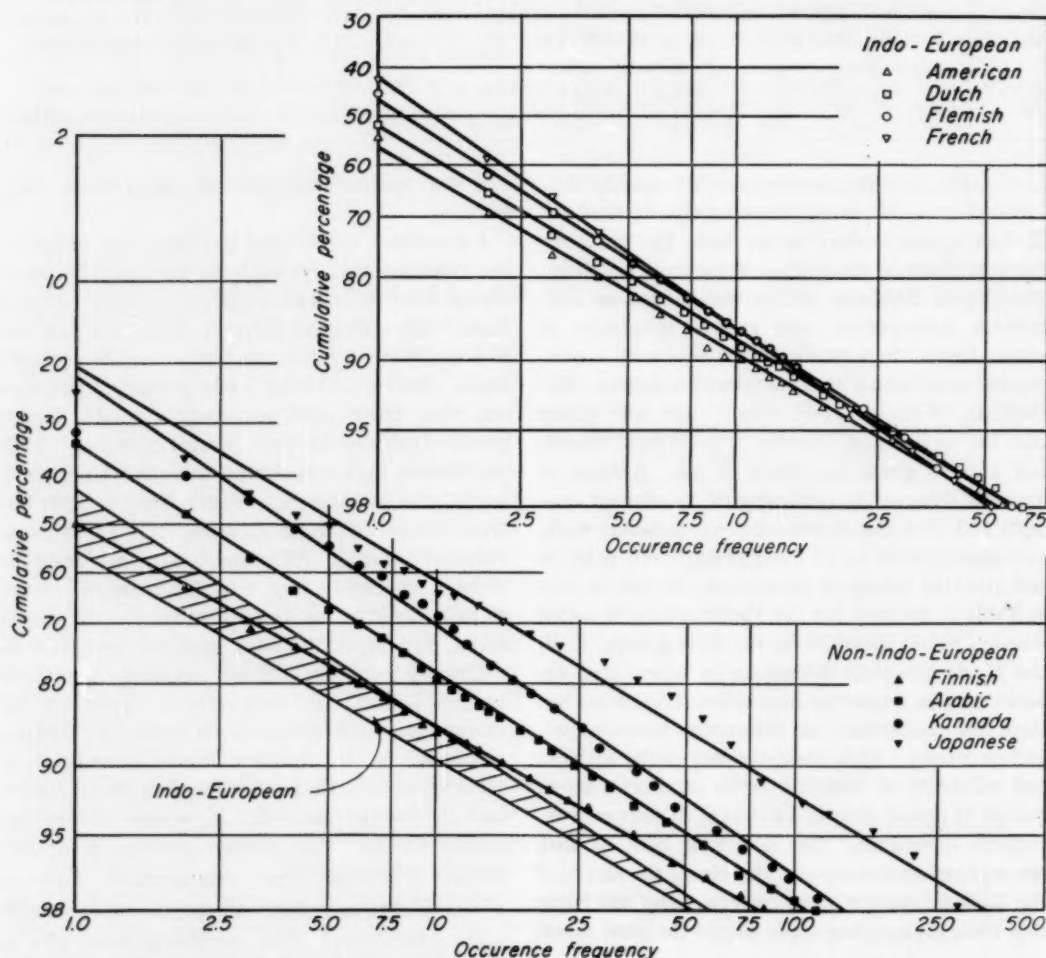


FIG. 3. Log-normal plots of frequency/diversity distributions for qualifiers from eight language/culture groups.

TABLE 4
TRANSLATION ANALYSIS ELICITED QUALIFIER INTERCORRELATIONS

	Frequency								Diversity							
	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
1. English	1.00	.53	.66	.78	.76	.43	.58	.65	1.00	.64	.56	.68	.70	.60	.53	.56
2. Arabic		1.00	.29	.35	.31	.39	.37	.34		1.00	.37	.58	.49	.46	.50	.40
3. Dutch			1.00	.73	.59	.29	.53	.95			1.00	.49	.48	.33	.41	.89
4. Finnish				1.00	.66	.39	.62	.71				1.00	.54	.56	.64	.55
5. Kannada					1.00	.33	.45	.56					1.00	.39	.46	.52
6. Japanese						1.00	.33	.34						1.00	.47	.36
7. French							1.00	.55							1.00	.42
8. Flemish								1.00								1.00

(b) when used as dimensions against which to rate a representative sample of concepts; (4) we would try to demonstrate that the factor structure remains essentially constant, (a) when bilinguals are compared in their two languages (to show that language code per se does not affect semantic factor structure) and (b) when monolinguals speaking different languages are compared (to show that cultural differences do not affect semantic factor structure).

In the large-scale cross-cultural project on which we are now working, Murray Miron is responsible for the statistical design and analysis and William Kay Archer is responsible for external cultural contacts and arrangements.³ We have already completed the sampling of qualifiers in eight countries. Figure 3 displays the frequency/diversity characteristics of adjectival qualifiers for each country, based on a sample of 100 young high-school males associating adjectives to 100 familiar nouns, as translated into their own language. These stimulus nouns were derived from an original list of 200 concepts selected as culture-fair by linguists and anthropologists, and the final 100 represent those which translate easily and consistently into all of our six language families as single terms. The functions shown are log-normal distributions of qualifiers for each country: if each subject had given a unique

response to each of the 100 stimuli, the function would be represented by a point at the lower left origin; if all subjects had given the same response, e.g., *good*, for all stimulus nouns, the function would be represented by a point off the graph to the far right. In other words, these functions indicate that, in all language-culture communities, a large number of adjectival qualifiers are used occasionally and a small number, like *good*, *big*, *happy*, are used frequently as modifiers of the standard 100 nouns. The commonness of slope shows that the familiarity/frequency law is general; the considerable differences in left-right allocation, e.g. between Finnish and Japanese, may indicate differences in vocabulary size, in cultural standardization, or some other variable.

The next question, obviously, is—are the *modes* of qualifying similar despite language-culture differences? That is, when translated into a common language (here, English), does the order of frequency in modes of qualifying correspond across groups? As shown in Table 4, the answer is clearly "yes"—all of the correlations shown here are highly significant beyond chance. Their magnitude indicates that the modes of qualifying experience, of differentiating aspects of events, tend to be the same regardless of what language one uses or what culture one has developed in. Of course, all of these correspondences are based on translations from language X into English, but this merely places an upper limit on the correlations.⁴

³ I wish to express my thanks to my administrative assistant, Howard M. Bobren; to the research assistants at the Institute of Communications Research, William H. May, A. V. Shanmugam, Yasumasa Tanaka, William Wexsel, and Sharon Wolfe; to our project secretary, Eileen Kaderavek; and to the University of Illinois Statistical Service Unit and Digital Computer Laboratory for the use of their facilities. Without the assistance of these people and the use of the computational equipment, practically none of the research reported in this paper could have been done.

⁴ Let me take this opportunity to express my gratitude to the many social scientists around the world who are cooperating in this research: Professor Obonai and Mr. Asai in Japan; Professor Kuppuswamy and Mrs. Shanmugam in Mysore, India; Dr. Li in Hong Kong; Miss Minou in Iran; Professors Prothro and Diab in Lebanon;

TABLE 5
HIGHEST LOADING SCALES ON FACTORS I-III; SCALE-ON-SCALE ANALYSIS, PRINCIPAL AXIS METHOD

Language	Evaluation	Potency	Activity
English	Factor I (49% CFV) ^a	Factor II (15% CFV)	Factor III (9% CFV)
	nice—awful .92	big—little .86	burning—freezing .81
	fine—coarse .92	powerful—powerless .81	hot—cold .76
	heavenly—hellish .91	strong—weak .77	fast—slow .66
	smooth—rough .91	long—short .75	sharp—dull .53
	mild—harsh .88	full—empty .67	light—dark .50
	clean—dirty .87	many—few .65	young—old .49
Dutch	Factor I (42% TV) ^b	Factor II (15% TV)	Factor III (10% TV)
	beautiful—ugly .93	impressive—insignificant .84	thin—thick .73
	pleasant—unpleasant .93	loud—soft .75	yellow—blue .70
	good—bad .92	big—little .73	loose—firm .61
	pretty—not pretty .92	strong—weak .72	fast—slow .55
	happy—unhappy .91	wild—tame .67	unexpected—expected .49
	tasty—dirty .91	much—few .67	new—old .49
Finnish	Factor I (47% CFV)	Factor II (11% CFV)	Factor IV (7% CFV)
	right—wrong .95	large—small .77	young—old .74
	honorable—despicable .94	deep—shallow .76	growing—diminishing .69
	good—bad .94	heavy—light .73	strong—weak .53
	valuable—worthless .93	difficult—easy .64	courageous—timid .50
	useful—useless .93	black—white .63	fast—slow .45
	clever—stupid .92	dark—light .63	glad—sad .44
Flemish	Factor I (42% CFV)	Factor II (11% CFV)	Factor III (10% CFV)
	agreeable—disagreeable .94	deep—shallow .78	violent—calm .81
	good—bad .94	serious—frivolous .73	impetuous—quiet .77
	magnificent—horrible .91	big—small .71	quick—slow .57
	beautiful—ugly .91	difficult—easy .66	strong—weak .57
	pleasant—boring .90	long—short .63	young—old .57
	clean—dirty .90	heavy—light .62	frequent—seldom .57
Japanese	Factor I (45% TV)	Factor II (16% TV)	Factor III (7% TV)
	pleasant—unpleasant .94	deep—shallow .86	cheerful—lonely .75
	good—bad .93	thick—thin .81	noisy—quiet .67
	happy—sad .93	complex—simple .68	near—far .62
	skillful—unskillful .90	strong—weak .68	hot—cold .53
	thankful—troublesome .90	sturdy—fragile .67	intense—calm .49
	agreeable—unagreeable .90	heavy—light .67	early—late .41
Kannada	Factor I (49% CFV)	Factor III (7% CFV)	Factor II (8% CFV)
	best—mean .94	big—small .73	fast—slow .83
	clear—unclear .92	wide—narrow .65	wonderful—ordinary .66
	soft—rough .91	huge—small .58	many—few .57
	pure—impure .90	great—little .55	red—black .54
	beautiful—ugly .89	plenty—few .54	public—secret .49
	delicate—rough .88	many—few .53	fatty—skin .45

^a CFV = % common factor variance. ^b TV = % total variance.

What about the factorial correspondence across these language-culture groups, when the 50 most

Professor Allardt and Mr. Haavio in Finland; Dr. Mathilda Jansen and Professors Duijker, Nuttin, and Pages in Holland, Belgium, and France; Professor Himmelstrand and Mr. Asplund in Sweden.

frequently used qualifiers and their opposites are made into scales and directly related to each other? Here I can only report on the data from five countries—as I said before, we are only part way through this project. Furthermore, the direct scale-on-scale method of collecting data tends to

magnify the dominant evaluative factor to such an extent that subsequent factors are obscured. However, as shown in Table 5 by the highest loading scales on a Varimax rotation of the Principal Axes factor analyses of the scale-on-scale data, we have clear evidence for a shared *evaluation* factor and a shared *potency* factor. The *activity* factor, so regularly reproducible in American data, is not so clear-cut cross-culturally—whether this means a difference in meaning systems or merely a happenstance of sampling scales in terms of frequency of usage is not yet determinable.

What about factorial correspondences when adjectival scales are used in rating various concepts, the usual "semantic differential" procedure? We have no data on this question from our major cross-cultural project as yet, but a number of earlier studies contribute suggestive evidence. First I will report a study done by H. Akuto⁵ and his associates in the Marketing Center Company, Tokyo, Japan—completely independent of our own research and therefore reasonably free of the kind of bias that made even rats in California run for Tolman and rats in Connecticut run for Hull! Akuto had some 100 monolingual Japanese subjects rate 90 concepts (subgroups of Ss rating subsets of concepts) against 50 scales in Japanese. Table 6 gives the Varimax rotations for these scales. There is the definite evidence for three corresponding factors, in order *evaluation* (comfortable, pleasant) *potency* (strong, masculine) and *activity* (lively, noisy, young). Independently obtained data like this are most encouraging.

Another study which approximates the ideal design was conducted by my colleague, George Suci (1960), in connection with the Southwest Project in Comparative Psycholinguistics, sponsored by the Social Science Research Council. A group of cooperating linguists and psychologists collected data on this and other problems from Navajo, Zuni, Hopi and other subjects in the American Southwest. The upper portion of Table 7 gives the loadings on the first two factors in terms of the scales deemed comparable from the original eliciting procedure. You will note that the scale loadings starred on the left indicate *evaluative* scales and those starred to the right indicate *potency* scales, for Factors I and II, respectively. The numbers in the lower portion of this table are the indices of factorial similarity—note particularly the itali-

cized values, which are the indices of similarity for field subjects and American controls who rated the *same concepts*, a necessary control when comparing factor structures across people. The remaining factors were not interpretable, but I should point out that here Suci was dealing with nonliterate subjects, studied individually via manual pointing procedures, and the reliability is not as high as for literate subjects using pencil-and-paper tests.

Table 8 compares factor loadings for Greek and American college students when judging a set of concepts against scales (Triandis & Osgood, 1958). The first factor is clearly *evaluation*—the highest loading scales for both groups being *positive-negative*, *good-bad*, *successful-unsuccessful*, and *timely-untimely*. But the second factor is *potency* for Americans and "dynamism" (a combination of potency and activity) for Greeks; the third factor is obviously *activity* for Americans. However, the distributions of variance and the indices of factorial similarity indicate a general sharing of semantic space. In this study, scales having known factorial characteristics for Americans were simply translated into Greek. Whether the difference in factor structure is due to differences in scale meanings (e.g., *angular-rounded*, *severe-lenient*), differences

TABLE 6
MARKETING CENTER CO., LTD. OF TOKYO STUDY:
UNROTATED CENTROID FACTOR LOADINGS FOR 5 HIGHEST
LOADING SCALES ON FIRST 3 FACTORS

	I	II	III	
comfortable	85	-20	09	uncomfortable
pleasant	83	-24	-07	unpleasant
bright	81	-20	-19	dark
good	76	-18	19	bad
beautiful	76	-25	21	ugly
strong	45	57	05	weak
masculine	11	53	-06	feminine
active	45	51	-20	passive
intense	-16	49	-37	mild
large	33	48	16	small
lively	-39	-07	49	lonely
noisy	26	-20	48	quiet
young	-44	08	47	old
shallow	24	23	44	deep
fast	-33	-28	40	slow
% Variance	30	09	06	

⁵ H. Akuto. Personal communication. 1960.

TABLE 7

SUCI DATA: FIRST TWO FACTORS (ROTATED) FOR NAVAJO, HOPI, ZUNI, AND ANGLO CONTROLS

Factor I					Scales	Factor II				
N	H	Z	ANH	AZ		N	H	Z	ANH	AZ
.07	.09	.11	-.04	.01	heavy*	.47	.45	.10	.47	.12
-.26	-.01	-.08	-.29	-.38	hard*	.26	.30	.23	.28	.45
.51	.72	.64	.61	.66	*pretty	.07	.01	-.06	-.06	.01
-.40	.20	.20	.01	.05	hot	.40	-.01	-.16	.00	.02
.34	.58	.65	.75	.72	*clean	.26	-.07	.03	-.05	.03
-.12	-.05	.12	-.20	-.29	dry	-.15	-.22	-.18	.01	.22
.41	.48	.16	.34	.17	*industrious	.01	.05	.67	.32	.66
.24	.53	.59	.39	.37	*happy	.42	.07	.08	-.06	.00
.00	.54	.60	.61	.59	*rich	.49	.03	.11	.24	.26
.32	.21	.33	.58	.63	*sweet	.02	-.08	.24	-.20	-.16
.05	.13	.23	.13	.14	long*	.48	.33	.31	.54	.36
.03	-.02	-.10	-.08	-.03	fast*	-.03	.53	.71	.60	.63
.12	.01	.02	.01	-.04	strong*	.46	.64	.72	.58	.54
.05	.06	.50	.53	.55	straight	.05	-.19	.05	.17	.12
.53	.42	.31	.69	.71	*good	.18	.05	.46	.02	-.02

Indices of Factorial Similarity						
	Factor I					
	Navajo	Hopi	Zuni	Anglo (Nav/Hop)	Anglo (Zuni)	
Navajo	—	.60	.42	.60	.45	Factor II
Hopi	.72	—	.74	.87	.70	
Zuni	.61	.51	—	.76	.80	
Anglo (Nav/Hop)	.79	.87	.91	—	.88	
Anglo (Zuni)	.79	.83	.88	.99	—	

in concept connotations (e.g., HOSPITAL, MOUNTAIN, and the like), or attributable to real differences in semantic framework is not clear in this case.

What about the influence of linguistic code per se upon semantic factor structure? In the earliest cross-cultural study in our series, Hideya Kumata (1956, 1957) had both Japanese and Korean bilinguals judge the same concepts against the same scales—first in English and then, a month later, in their native language for half of the subjects in each group, and the reverse order for the other half. A group of American college students, taking the same test twice in English over the same interval, served as control. The upper portion of Table 9 gives the indices of factorial similarity, for Factor I (below diagonal) and Factor II (above diagonal)—these factors being *evaluation* and *dynamism* respectively. Note that J_j (Japanese in Japanese) vs. J_e (Japanese in English) indices,

and the K_k (Koreans in Korean) vs. K_e (Koreans in English) indices, are of the same magnitude as the A_1 vs. A_2 (American test-retest) indices—and all values are nearly unity. In other words, Japanese and Korean bilinguals, when judging the same concepts and scales in two language codes differ in semantic factor structure no more than American monolinguals do when taking the same test twice in their one language. I conclude that the language code, per se, has no influence upon semantic factor structure, upon the space within which meaningful judgments are made. The A/J line in the lower half of this table gives similar data for monolingual Japanese vs. Americans; the correspondences are a little lower, but still significant. The remainder of the table reports male and female comparisons. These monolingual data were based on simple translation of American scales into Japanese, however, and therefore do not satisfy the requirements of ideal design.

SPECULATIONS ON THE "WHY" OF GENERALITY
ACROSS PEOPLE

The evidence to date, then, indicates considerable generality in semantic space across people, both within and between language—culture groups. Now let me speculate a bit on the "why" of this generality. First, I must confess that, when we began this research over ten years ago, I had the expectation that the major factors of the semantic space would represent the ways in which our sensory apparatus divides up the world—e.g., would parallel Boring's "dimensions of consciousness." This was in flat contradiction to my own mediation

theory of meaning, in which the semantic components ought to be *response-like* dimensions. The accumulating data have proved my expectation wrong and my theory at least "righter"—the dominant factors of *evaluation*, *potency*, and *activity* that keep appearing certainly have a response-like character, reflecting the ways we can react to meaningful events rather than the ways we can receive them.

But these major factors also seem to have an *affective* as well as a response-like character. As a matter of fact, the similarity of our factors to Wundt's (1896) tridimensional theory of *feeling*—

TABLE 8

GREEK STUDY: QUANTIMAX ROTATION OF FIRST FOUR FACTORS AND INDICES OF FACTORIAL SIMILARITY

Scales	Factors					
	I		II		III	
	G	A	G	A	G	A
high—low	.57	.88	.53	.15	-.30	-.05
slow—fast	-.02	.13	-.80	-.29	-.33	-.76
important—unimportant	.83	.67	.34	.31	-.03	.14
negative—positive	-.95	-.98	-.21	.02	-.01	-.06
grateful—ungrateful	.93	.86	-.07	-.31	-.07	-.26
heterogeneous—homogeneous	-.65	-.42	-.03	.38	-.28	.21
good—bad	.92	.94	-.20	-.08	-.10	-.15
excitable—calm	-.69	-.39	.36	-.06	.28	.72
optimistic—pessimistic	.88	.91	.13	-.21	.24	.08
unsuccessful—successful	-.97	-.96	-.12	.01	-.03	-.04
timely—untimely	.91	.91	.17	.02	.14	-.07
egocentric—altruistic	-.91	-.78	.09	-.09	.07	.33
sociable—unsociable	.86	.80	-.03	-.24	.15	.15
sickly—healthy	-.84	-.78	-.25	-.03	-.03	-.30
hard—soft	-.35	-.29	.70	.90	-.35	-.14
light—heavy	-.07	.20	-.53	-.83	.62	.30
sober—drunk	.77	.63	-.27	.06	-.10	-.65
feminine—masculine	-.15	.07	-.84	-.85	.07	-.13
severe—lenient	-.11	-.42	.69	.75	-.10	.06
strong—weak	.37	.49	.86	.76	-.10	.21
old—new	-.43	-.25	-.17	.25	-.68	-.71
colorful—colorless	-.02	.51	-.16	-.19	.17	.50
unusual—usual	-.61	-.52	.09	.04	-.12	.33
private—public	-.19	.13	-.36	-.62	.44	-.20
active—passive	.55	.25	.73	.35	.17	.78
cold—hot	-.19	.09	-.06	.06	-.63	-.47
changeable—stable	-.72	-.51	-.24	-.32	.46	.69
angular—rounded	.90	.41	.33	.83	-.06	.00
ugly—beautiful	-.77	-.83	.05	.28	-.06	-.09
straight—curved	.52	.28	.61	.80	.40	.04
% Total Variance	.43	.39	.17	.17	.08	.16
Index Factorial Similarity	.93		.81		.60	

TABLE 9
KUMATA STUDY: INDICES OF FACTORIAL SIMILARITY

Factor II	Bilingualism Study ^a					
	Factor I					
	A ₁	A ₂	J ₁	J _e	K _k	K _e
	A ₁	.987	.929	.931	.885	.904
	A ₂	.983	.913	.905	.872	.889
	J ₁	.949		.977	.943	.961
	J _e	.938	.989		.938	.960
	K _k	.906	.971	.979		.983
	K _e	.892	.956	.957	.980	
	Monolingual Study ^b					
		Factor I	Factor II	Factor III		
	A/J	.94	.86	.76		
	AM/AF	.99	.98	.96		
	JM/JF	.99	.93	.91		
	AM/JM	.93	.86	.68		
	AM/JF	.93	.83	.76		
	AF/JM	.93	.86	.70		
	AF/JF	.91	.80	.73		

^a Americans 1st testing, A₁; Americans 2nd testing, A₂; Japanese subjects in Japanese, J₁; Japanese subjects in English, J_e; Korean subjects in Korean, K_k; Korean subjects in English, K_e.

^b Americans, A; Japanese, J; Males, M; Females, F; all own language. Lower limit of significant agreement, .76.

pleasantness-unpleasantness, strain-relaxation, and excitement-quiescence—has been pointed out to me.⁶ A dimensional analysis of the labels used to describe 40 live-posed facial expressions (Osgood, 1955) further substantiates this relationship; three factors—*pleasantness*, *control* (or *intensionality*) and *activation*—accounted for a large share of the variance. Later and as yet unpublished experiments by Albert Hastorf and myself⁷ have confirmed the stability of these factors in the semantic-differential judgments of 35 posed-and-photographed facial expressions.

These dimensions correspond pretty well with those reported by Harold Schlosberg and his associates (1952, 1954, 1957)—*pleasantness-unpleasantness*, *sleep-tension*, and *rejection-attention*. Engen and Levy (1956), and Triandis and Lambert (1958) working with Greek subjects, have shown that these Schlosberg dimensions can be used reliably, but it is a somewhat a priori basis

on which they were derived. Abelson and Sermat⁸ have used a multidimensional scaling method in which absolute judgments of global similarity are obtained for all possible pairs of facial stimuli; of five dimensions extracted, the first two account for 73% of the total variance, and they were identified as *pleasant-unpleasant* and either *sleep-tension* or *rejection-attention*, these two Schlosberg dimensions being about equally correlated with their Factor II. Eckman (1955) has used a very similar method in studying the semantic structure of 23 Swedish words for emotional states; of some nine factors extracted, all of which seem to be unipolar, the first two are *pleasure* and *discomfort*, while the third is *agitation*.

The similarities between the factors obtained from judging facial expressions and those obtained in our more general linguistic studies suggest that the latter also may have their grounding in the affective system. Let me speculate a bit further

⁶ M. Brewster Smith. Personal communication.

⁷ C. E. Osgood and A. Hastorf. A semantic structure of facial communication. Unpublished study, 1961.

⁸ R. P. Abelson and V. Sermat. Multidimensional scaling of facial expressions. Unpublished study, privately distributed. 1961.

and suggest that the highly generalized nature of the affective reaction system—the fact that it is independent of any particular sensory modality, yet participates with all of them—is at once the mathematical reason why *evaluation*, *potency*, and *activity* tend to appear as dominant factors and the psychological basis for synesthesia and metaphor. It is *because* such diverse sensory experiences as a *white* circle (rather than a black), a *straight* line (rather than crooked), a *rising melody* (rather than a falling one), a *sweet* taste (rather than a sour one), a *caressing* touch (rather than an irritating scratch) can all share a common affective meaning that one can easily and lawfully translate from one modality into another in synesthesia and metaphor. This is also the basis for the high interscale communalities which determine the nature and orientation of general factors. Speculating still further, I would suggest that this meaning system is intimately related to the nonspecific projection systems from the hypothalamic, reticular, or limbic systems and their cortical connections in the frontal lobes—both are gross, nondiscriminative, but highly generalizable systems and both are associated with the affective, purposive and motivational dynamics of the organism.

I have as yet only some incidental and entirely inadequate evidence on aphasics to support the last speculation—despite gross disturbances in labeling and other denotative linguistic tasks, the few patients I studied⁹ seemed to have no impairment in appropriate affect, and they made some synesthetic judgments essentially like normals (aphasia is associated with lateral rather than frontal lesions). But I do have some relevant data on the cross-cultural generality of synesthetic and metaphorical tendencies.

One study dealt with *visual-verbal synesthesia* in Navajo, Mexican-Spanish, Japanese, and American subjects, the research being partially supported by the Southwest Project in Comparative Psycholinguistics (Suci, 1960). Each of the 13 pairs of visual forms shown in Figure 4 was presented on a single card and differed in only one visual dimension, as indicated by the labels given here (but not in the experiment). A subject would be given a word in his own language, e.g., *HAPPY*, and then asked to simply point to the side of each card which

⁹ C. E. Osgood and M. S. Miron (Eds.) Approaches to the study of aphasia. Unpublished study, privately distributed. 1958.

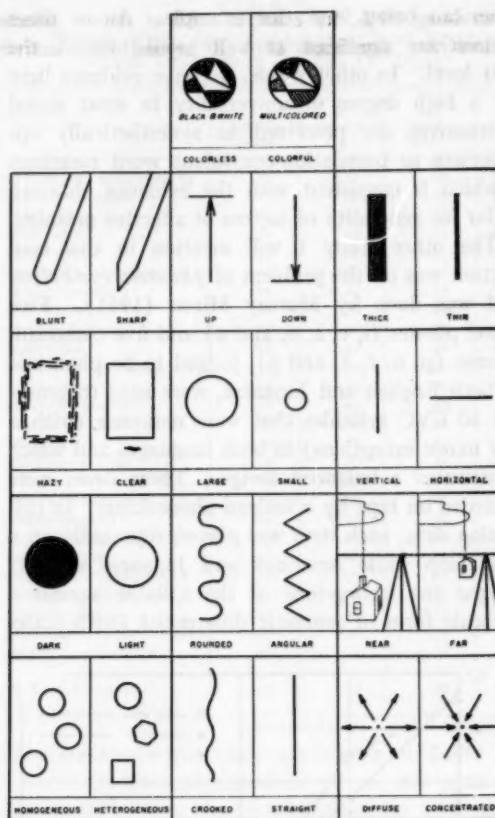


FIG. 4. The 13 visual polarities used in a cross-cultural visual-verbal synesthesia experiment.

seemed to go best meaningfully with the word—for example, for *HAPPY* he might typically point to the *colorful* side, the *sharp* side, the *clear* side, the *light* side, and so forth. The first observation worth reporting is the high degree of agreement over subjects *within* each language-culture group: for all groups nearly 50% of the 364 items (28 verbal concepts judged against 13 visual alternatives) showed intracultural agreements significant at the .01 level. What about cross-cultural agreement? The fairest test is to take just those items where *both* groups being compared show significant *intracultural* agreement in direction and then ask what percentage of such items also show *intercultural* agreement—that is, show the same direction of choice. Applying this test, we find that Navajo and American subjects agreed on 87% of such items and that all other group comparisons yielded agreements above 90%, Japanese with

American being 99%, for example. All of these proportions are significant at well beyond the .001 level. In other words, we have evidence here for a high degree of universality in what visual alternatives are perceived as synesthetically appropriate to translation-equivalent word meanings—which is consistent with the evidence obtained so far for generality of factors of affective meaning.

The other study I will mention in this connection was on the problem of *phonetic symbolism* and was done by Murray Miron (1961). Five vowel phones (i, e, a, o, and u) and five consonant phones (p, n, t, s, and g), judged to be phonemic in both English and Japanese, were used to generate 50 CVC syllables that were nonsense (with a few minor exceptions) in both languages and which constituted a balanced design. These items were recorded on tape by a trained phonetician. In collecting data, each item was played repeatedly on a tape loop while American and Japanese subjects judged the feeling-tone of the syllable against a 15-scale form of semantic differential (with scales

selected to represent *evaluation*, *potency* and *activity* as well as certain physical dimensions). The first result of interest is that the Japanese and American factors found here with nonsense syllables as stimuli corresponded significantly and turned out to be our old friends, E, P, and A. A second finding of interest is illustrated in Figure 5, which displays the correspondences between Japanese and American mean judgments on the potency factor for vowels (upper graph) and consonants (lower graph). The correlations between groups were .57 and .91 for vowels and consonants respectively, both significant at beyond the .05 level. Similar correlations for evaluative and activity meanings of speech sounds were also significant, with the one exception of the evaluation of vowel sounds. In other words, under pretty stringent experimental conditions, languages as different as Japanese and English are found to share common tendencies toward phonetic symbolism. The third point of interest is indicated in the upper portion of Figure 5 by the dashed line: For both groups, although for Americans more clearly, judged potency tends to follow the second vowel formant, i.e., high frequencies tend to be associated with smallness and impotence. An otherwise similar study by Solomon (1959), using sonar signals, reports the same relation. Another finding was that front consonants are more pleasant than back (e.g., /p/ vs. /g/), for both groups.

GENERALITY ACROSS CONCEPTS

Now, let us flip the coin over and ask about the generality of semantic factor structures across the concepts being judged. You will recall that the cube of data generated when a group of subjects judges a sample of concepts against a set of scales makes it feasible to compute separate correlation matrices for each concept "slice" (i.e., r 's based on interscale cross-products over subjects) and factorize such matrices. In what we refer to as our "Thesaurus Study"—because the adjectival scales were sampled from that source on a rational, representative basis—20 different concepts, like FOREIGNER, KNIFE, MODERN ART, DEBATE, and HOSPITAL, were judged against 76 scales by 100 college subjects (Osgood, Suci, & Tannenbaum, 1957, Ch. 2).

Now, imagine the 20 separate correlation matrices for the different concepts lined up as a deck; if we go through the deck at the point of intersec-

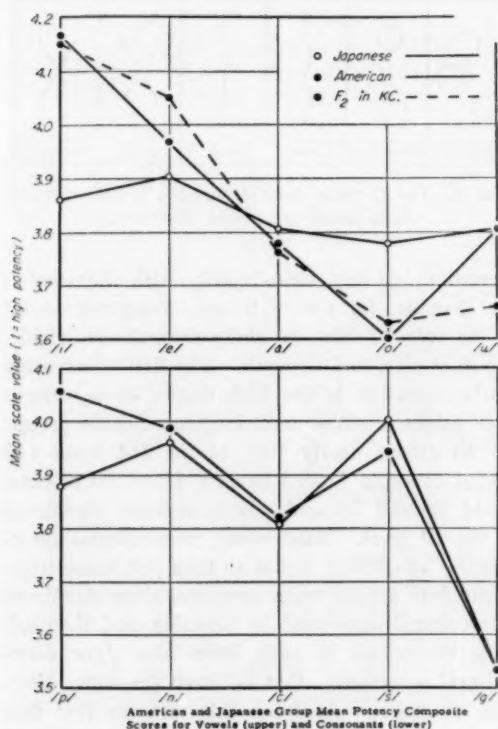


FIG. 5. Mean/potency factor scores for American and Japanese subjects judging auditory nonsense items: composites for vowels (upper) and consonants (lower).

tion of a particular pair of scales (e.g., of *sober-drunk* vs. *mature-youthful*), we will isolate 20 *r*'s, one for each concept. If scale relations were reasonably constant over concepts, then we would expect only minor variations within such rows of correlations—but this proved *not* to be the case. Corresponding *r*'s were found to vary as much as from $+.60$ to $-.60$ in the same row. A couple of examples will serve to suggest what is happening: *sober* goes with *youthful* for the concept DAWN, but with *mature* for the concept UNITED NATIONS; *pleasurable* goes with *feminine* for the concept MOTHER, but with *masculine* for the concept ADLAI STEVENSON. It would appear that the nature of concept being judged exercises a *denotative restriction* on scale meaning—I will come back to this point. What about the correspondence of factors derived from such single-concept matrices? Here the picture is better: something identifiable as an *evaluative* factor appeared for each concept, and it was usually the first in order of magnitude; something identifiable as a *potency* (or *dynamism*) factor appeared for all but two concepts; but other factors varied among what we call *activity*, *stability* and *receptivity* in most inconsistent ways.

This instability of scale relations and factors across concepts contrasts sharply with the stability we have found across people. This shows up most clearly in several studies where both types of generality can be compared. In a study on the 1952 election, conducted by George Suci (Osgood, Suci, & Tannenbaum, 1957, Ch. 3), separate analyses were made for three types of voters, Stevenson Certain, Eisenhower Certain, and Taft Certain, judging the same 20 politically relevant concepts against the same set of 10 scales. Although, as would be expected, these different voter groups differed markedly in their meanings (localizations in the factor space) of such concepts as TRUMAN, SEN. MCCARTHY, and FEDERAL SPENDING, their scale factor systems were almost identical—but for these political concepts the three usually independent factors of evaluation, potency, and activity were fused into a single, dominant factor describable as *benevolent dynamism* vs. *malevolent insipidity*. A similar fusion of our three factors into one was found when Stanford college students rated Charles Morris's "ways to live" (various human values) against a 26-scale differential (Osgood, Ware, & Morris, 1961)—here the fused factor was best identified as *successful-unsuccessful*.

TABLE 10
MORRIS STUDY: QUARTIMAX ROTATION OF SCALE
FACTORS OBTAINED IN JUDGING 13 "WAYS"

	I "success- fulness"	II "stabil- ity"	III "socia- bility"	IV ?
interesting—uninteresting	.96	-.15	.08	.11
timely—untimely	.97	.02	.10	.18
new—old	.81	-.43	-.13	.11
kind—cruel	-.06	.34	.92	-.10
hard—soft	.54	-.02	-.82	.11
successful—unsuccessful	.98	.08	.00	.14
active—passive	.91	-.32	-.18	.00
true—false	.92	.20	.14	.07
savory—tasteless	.92	-.30	.18	.06
good—bad	.79	.33	.49	-.10
masculine—feminine	.75	-.25	-.56	-.07
quick—slow	.84	-.45	-.26	-.10
stable—changeable	-.40	.84	.30	.19
angular—rounded	.02	-.30	-.83	-.40
warm—cool	.18	-.31	.85	-.14
straight—curved	.43	.70	-.35	-.27
colorful—colorless	.80	-.56	.09	-.04
positive—negative	.95	.02	.27	-.10
usual—unusual	.66	-.03	.36	.56
beautiful—ugly	.46	.09	.85	-.08
strong—weak	.93	.08	-.31	-.14
important—unimportant	.93	.25	-.01	-.17
excitable—calm	.71	-.64	-.23	-.08
wise—foolish	.85	.44	.21	.06
predictable—unpredictable	-.20	.89	.29	.20
powerful—powerless	.96	.11	-.18	.10
% Total Variance	.57	.16	.20	.03

The results of this study are shown in Table 10.

An experiment designed to get at this problem has recently been conducted jointly by T. Oyama of Hokkaido University and Y. Tanaka of our Institute staff.¹⁰ College girls in both Japan and the United States rated three different classes of concepts—patches of color, line forms, and abstract words—against a common 35-scale form of translation-equivalent differential. Separate scale-by-scale correlation matrices, factor analyses and Varimax rotations were computed for each subject group/concept-class combination, six in all. Table 11 presents the correlations among original correlation matrices as one measure of over-all similarity; thus, if the scale-by-scale correlations for Japanese girls judging Colors (J_c) tend to be similar to those for American girls judging Colors (A_c), then correlating their two matrices across corresponding cells will yield a high value. If our hypothesis holds—that generality is higher across subject groups than across concept classes—then the critical italicized values (J_c/A_c ;

¹⁰ T. Oyama and Y. Tanaka. A cross-culture and cross-concept study of the generality of semantic spaces. Unpublished study, privately distributed. 1961.

TABLE 11

OYAMA-TANAKA STUDY: CORRELATIONS AMONG SCALE-ON-SCALE CORRELATION MATRICES

	J_e	J_t	J_w	A_e	A_t	A_w
J_e	*	.51	.33	.62	.43	.21
J_t		*	.41	.32	.52	.25
J_w			*	.36	.28	.65
A_e				*	.43	.41
A_t					*	.24
A_w						*

J_t/A_t ; and J_w/A_w) for the two groups judging the same concepts should be higher than any other values in the same rows or columns. This is clearly the case. This result is particularly impressive in this instance because, as Table 12 shows, the first three factors in all analyses were identifiable as *evaluation*, *potency*, and *activity*, although with somewhat different scales involved and with definitely different orderings of variance extracted. Note that for Colors an activity factor is most salient for both groups (that is, accounts for the

highest % Variance), while for Abstract Words an evaluation factor is dominant for both; potency is clearly most salient for Japanese when Visual Forms are judged, but not so clearly for Americans. Looking down the highest loading scales for each factor (e.g., potency) for both groups, one can see how the semantic "flavor" of the factor shifts from concept class to concept class, but again similarly across subject groups.

SPECULATIONS ON THE "WHY" OF CONCEPT/SCALE INTERACTION

The preceding evidence as a whole forces us to the conclusion that *there is a significant interaction between concepts and scales in the process of semantic judgment*. What are the implications of this? From the standpoint of the practice of semantic measurement, this means that there is no such entity as "The Semantic Differential," with a rigidly defined set of factors—except perhaps in the sense of a common denominator from which more specific instruments are to be derived. For significant concept classes we will therefore want

TABLE 12

OYAMA-TANAKA STUDY: HIGHEST LOADINGS SCALES ON VARIMAX ROTATED FACTORS AND % VARIANCE

	E		P		A	
	Japanese	American	Japanese	American	Japanese	American
Colors	fresh (.95)	beautiful (.92)	strong (.94)	deep (.96)	excitable (.96)	fast (.97)
	new (.92)	good (.87)	tense (.86)	heavy (.95)	gay (.94)	excitable (.95)
	clear (.90)	real (.85)	distinct (.82)	strong (.95)	dangerous (.90)	sharp (.93)
	good (.88)	healthy (.83)	deep (.81)	hard (.92)	near (.89)	dynamic (.90)
	healthy (.87)	fresh (.74)	full (.78)	dark (.91)	dynamic (.87)	distinct (.88)
	% Variance .23	.24	.22	.28	.34 *	.32*
Forms	full (.92)	healthy (.88)	hard (.98)	angular (.97)	interesting (.90)	happy (.87)
	light (.91)	clear (.86)	angular (.98)	sharp (.96)	characteristic (.82)	fresh (.86)
	good (.88)	good (.84)	manly (.94)	tense (.94)	new (.80)	intellectual (.85)
	healthy (.88)	ordered (.82)	cold (.94)	manly (.90)	dynamic (.54)	gay (.84)
	stable (.87)	real (.76)	sharp (.91)	hard (.89)	gay (.50)	new (.83)
	% Variance .29	.22	.36*	.25	.11	.29*
Words	light (.97)	light (.95)	real (.87)	near (.89)	dynamic (.89)	dynamic (.93)
	fresh (.97)	smooth (.95)	distinct (.84)	distinct (.89)	characteristic (.56)	fast (.92)
	healthy (.97)	beautiful (.95)	near (.81)	real (.64)	excitable (.48)	wet (.74)
	beautiful (.94)	good (.95)	strong (.69)	strong (.53)	gay (.47)	sharp (.72)
	clear (.93)	healthy (.94)	dry (.68)	clear (.48)	fast (.43)	excitable (.70)
	% Variance .49*	.56*	.15	.13	.08	.15

to develop specific instruments, and for the important class of personality concepts we have already done some work. From the standpoint of the theory of language and cognition, this conclusion invites fresh speculation about the principles that may govern such concept/scale interaction—and a whole host of new experiments. Here we are just beginning, and I will close this paper with some of our speculations on the matter.

But first, let me tell you briefly about the attempt that Edward Ware and I have been making to develop a *Personality Differential*. In the Thesaurus Study I mentioned earlier, one of the concepts for which we did a separate factor analysis was the self-concept, ME. Six rather intriguing factors were indicated, and on this basis—and with the help of Wesley Becker—a pilot study was run in which six “personality” concepts (ME, MY MOST LIKED SELF, MY MOTHER, MY FATHER, MY BEST FRIEND, and ADLAI STEVENSON) were rated by 45 college subjects against 30 scales, these being derived from the earlier Thesaurus self-concept factors and our own heads. We wanted to see if a set of scales, representing identifiable factors, could be shown to hold their factorial structures stably across these concepts. So separate factor analyses were run for each of the six concepts and the results compared. The three factors which held up best were variants on our old friends, but here identified as *morality* (moral-immoral, reputable-disreputable), *volatility* (excitable-calm, emotional-unemotional), and *toughness* (hard-soft, masculine-feminine). Three other factors, less clearly isolated, were dubbed *sociability*, *uniqueness*, and *tangibility*.

Encouraged by these preliminary results, we solicited and obtained a grant from the National Institute of Mental Health to continue this line of work. As a next step, we took two independent samples of 34 general personality traits from the large domain provided by Allport and Odbert in their monograph on trait names (1936), and to each sample we added six common marker variables from the previous factorial results. Rather than having personality concepts judged against these scales, we simply had subjects rate each scale against every other, in the form:

A person who is:
mature (not immature)

Is likely to be:

logical—:—:—:—:—impulsive

The factor analyses of these two samples of scale-on-scale judgments yielded such large evaluative factors (58% and 69% of total variance, respectively) that little else could be determined, except that additional *volatility* and *uniqueness* factors were discernible in both cases. We think that this result was due both to the scale-against-scale procedure (in which even slight evaluative relations tend to be magnified) and to the fact that a large proportion of traits in the personality “pool” we were sampling are primarily modes of evaluation.

In the most recent experiment of this series we assembled a sample of 40 scales, some to represent previously suggested factors and the remainder to deliberately represent as many seemingly independent dimensions as possible; we were simultaneously trying to damp evaluation and force out new factors if they could be found. We then assembled 40 diverse “personality” concepts—kin terms (like MY MOTHER and MY WIFE, or HUSBAND), various roles of the self (like MYSELF AS A PARENT and MYSELF AS A FRIEND), other known persons (like A CLOSE MALE FRIEND and A PERSON WITH WHOM I HAVE WORKED), various occupational stereotypes (like A DOCTOR and AN ARTIST), and some animal “personalities” (like A CAT, A SPIDER, and A LAMB). We had 20 married college adults (10 male and 10 female) rate all 40 concepts against all 40 scales—an arduous task for which they were well paid. Table 13 gives the first eight factors in order of variance extracted, based on factoring and rotating the matrix of average intercorrelations for the 20 subjects. Note that all of the factors identified in the previous studies reappear here—*morality*, *excitability* (or *volatility*), *toughness*, *sociability*, *uniqueness*, and *tangibility*—along with two newcomers, *rationality* and some difficult-to-name Factor VII. Note also that the proportions of variance accounted for by these factors are roughly equal in magnitude, together accounting for 50% of the total variance.

The fact that very similar factors keep appearing in these studies suggests that there may be a common semantic system within which personalities are described. But is this system really a shared “theory” of personality or is it merely the result of averaging over quite dissimilar individual spaces? To answer this question, the correlational matrices for the individual subjects were correlated with each other by corresponding cells and the resultant subject/subject matrix factored. Now,

TABLE 13
VARIMAX ROTATION FOR EIGHT FACTORS IN PERSONALITY
DIFFERENTIAL (20 SUBJECTS BY 40 CONCEPTS BY 40 SCALES)

Factor I	7.9% of variance	
<i>Morality (E)</i>		
moral—immoral		78
reputable—disreputable		78
wholesome—unwholesome		73
Factor II	7.1% of variance	
<i>Rationality</i>		
logical—intuitive		66
objective—subjective		66
rational—irrational		60
Factor III	6.7% of variance	
<i>Uniqueness</i>		
unique—typical		77
unusual—usual		74
individualistic—regular		70
Factor IV	6.6% of variance	
<i>Excitability (A)</i>		
excitable—calm		81
tense—relaxed		77
emotional—unemotional		52
Factor V	6.5% of variance	
<i>Sociability</i>		
gregarious—self-contained		76
sociable—solitary		72
extroverted—introverted		66
Factor VI	6.0% of variance	
<i>Toughness (P)</i>		
tough—tender		78
insensitive—sensitive		71
rugged—delicate		63
Factor VII	5.2% of variance	
proud—humble		65
sophisticated—naive		58
deliberate—casual		53
Factor VIII	4.0% of variance	
<i>Tangibility</i>		
formed—amorphous		72
predictable—unpredictable		56
tangible—intangible		42

if all 20 people related scales in exactly the same ways when judging the 40 personality concepts, then correlations among all subject/subject matrices would be unity, and a single factor accounting for 100% of the variance would result; if each person had a unique way of relating scales, then 20 specific factors would emerge. The results of

this analysis were entirely convincing: one factor accounted for 63% of the total variance and the next factor in order accounted for only 6%. In other words, these 20 people do share essentially the same "theory" for describing personality. But is the same factor system general across the domain of personality concepts? To answer this question it will be necessary to do factor analyses for separate concepts and compare them in the same fashion. We have not done this as yet.

Finally, speculations on the "why" of concept/scale interaction. I start with the notion of an affective mediating system which is biologically determined and capable of some limited number of gross, bipolar discriminations. This is the system the semantic differential technique is assumed to tap primarily; I have referred to the aspect of meaning indexed as *connotative*. But there is another aspect of the meaning of signs which I refer to as *denotative*: This is the elaborate set of essentially arbitrary correlations between non-linguistic and linguistic events, e.g., between the visual perception of APPLE object and vocalizing the word "apple." I assume that these correlations are mediated by the sensory and motor discrimination systems of the brain—regions where lesions produce various types of aphasia. Both of these biological systems—the affective energizing system and the sensory-motor discrimination system—are integrated in ordinary behavior, as we know, and I think integration of the same systems in language behavior is one of the reasons for concept/scale interaction.

I have been criticized for this usage of the terms "connotation" and "denotation" by a number of linguists, psycholinguists, and philosophers (cf. Weinreich, 1958; Carroll, 1959; Osgood, 1959), but I think the distinction and perhaps even the usage is justified. If I were to ask you the question, Is a BABY *large* or *small*?, you would undoubtedly say "small." And if I were to ask, Is a railroad SPIKE *large* or *small*?, you would probably say "large." Within the class of human organisms, a baby is "a small one," and within the class of nails a spike is "a large one." I think the semantic differential technique is one which tends to draw out these connotations of signs; it is a procedure in which a single stimulus is judged successively against a series of different scales. In all other psychophysical methods with which I am familiar, even the so-called "absolute judgment"

methods, a series of different stimuli are judged successively against a single scale—e.g., in judging weights, in scaling the loudness of tones, and so forth—and I think this judgmental situation tends to draw out the denotative meaning of scales. Note that if I ask you, Is a BABY *larger* or *smaller* than a SPIKE?, you immediately say “larger,” now disregarding the within-class connotations of these objects. We have just begun a series of experiments comparing these two methods of “absolute judgment,” and I believe we will be able to demonstrate that many of the standard phenomena of the traditional method, such as “anchoring effects,” do not appear in the semantic differential procedure.

What has all this to do with concept/scale interaction? I think the semantic differential is subject to what might be called *denotative contamination*. Most adjectival scale terms have variable denotative meanings as well as their affective connotation. Particular concepts exert a selective limitation upon scale meanings, drawing forth a denotative usage of some and the connotation of others. The denotation of *masculine-feminine* is elicited by the concept ADLAI STEVENSON while its potency connotation is elicited by the concept DYNAMO; a denotation of the scale *hot-cold* is tapped by the concept LAVA, whereas its activity connotation is tapped by concepts like JAZZ and FESTIVAL. It is clear that if certain scales are denotatively relevant to a certain class of concepts, they will tend to fall out of the general affective factors with which they usually correlate. I am hopeful that comparisons between the two psychophysical methods mentioned above—the traditional method eliciting denotative judgments and the semantic-differential method eliciting connotative judgments—will enable us to disentangle these two confounded processes.

Another cause of concept/scale interaction is what might be called *factorial coalescence*. The general factors we obtain with large and diverse sets of concepts presumably indicate the major ways in which the affective representational system *can* make discriminations. However, it may be characteristic of certain classes of concepts that two or more of these potentially independent factors just simply are correlated, and hence coalesce mathematically. But what is the dynamism that produces such correlations? I think it is a special instance of *cognitive interaction*, a phenomenon

studied by Heider (1958) as “imbalance,” by Festinger (1957) as “dissonance,” and by Tannenbaum and myself (1955) as “incongruity.” Our own studies—on attitude change, on word mixture and on facial fusion—are most directly relevant, because we have dealt with change in meaning of the interacting elements as one of the resolutions of cognitive stress. The basic notion is this: whenever two or more signs are presented near-simultaneously, the representational mediating reactions characteristic for each must interact and reach a compromise—this is because the affective meaning system is capable of assuming only one “posture” at a time; the congruity hypothesis states that, in such interactions and independently on each factor, the change in meaning for each sign is inversely proportional to its initial polarization or intensity—that is to say, the more intensely meaningful signs are modified less.

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So you see, we still have to report, as we did in the last chapter of *The Measurement of Meaning*, that our work is “in progress” and that “more

research is needed," but this is as it should and must be. As has been evident in this paper, the work to date has been a collaborative effort on the part of many people, not only on our own staff but also in other universities here and abroad. This is a good opportunity to thank them all for their interest and their help.

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THE SURPRISING SIMPLICITY OF SENSORY METRICS

S. S. STEVENS

Harvard University

IF you shine a faint light in your eye, you have a sensation of brightness—a weak sensation, to be sure. If you turn on a stronger light, the sensation becomes greater. Clearly, then, there is a relation between perceived brightness and the amount of light you put in the eye. The visual sense organ behaves as a transducer with an operating characteristic of some kind—an input-output function. But how, precisely, does the output of the system (sensation) vary with the input (stimulus)? Suppose you double the stimulus, does it then look twice as bright?

The answer to that question happens to be no. It takes about nine times as much light to double the apparent brightness, but this specific question, interesting as it may be, is only one instance of a wider problem: what are the input-output characteristics of sensory systems in general? Is there a single, simple, pervasive psychophysical law?

Unlikely as it may seem, there appears to be such a law. Its form is a power function, and not the logarithmic relation that is almost universally cited in textbooks. The power law, although not yet widely known, is becoming so well fortified with evidence that it may someday replace the older law in all discussions of sensory dynamics. The wonder is, in fact, how we could have missed finding the power law for so long, especially since it is so surprisingly simple to demonstrate.

I pause at this point to pay respect, as is the custom, to the engrossing pursuit we call science—that relentless intellectual scrimmage which means such diverse things to its many devotees. Some regard it as a quest, a nomological quest, wherein the quarry reveals itself from time to time in the form of simplicities and uniformities in the complex of nature. Others regard the canons of impersonal, nomothetic endeavor as oppressive strictures against the free-wheeling development of an ideographic, personal science. These two polar points of view have always been with us—the tough- and tender-minded, as William James called them; the hircine and the ovine, as E. G. Boring suggests

they be named—and the clash of their differing value systems will no doubt resound until the final éclat. My own guess is that even a phenomenological existentialist would cherish a natural law if he found one. Perhaps the psychologists' questing for nomological principles ought to get extinguished for lack of frequent reinforcement, but obviously it does not. Maybe it comes with the organism, like native curiosity, wired in from the start. Whatever it is that motivates inquiry, we can be sure that psychologists will not renounce the experimental search for simple and powerful principles of behavior.

The discovery of a law does not put an end to the nomological pursuit. At least it never has. There seems in fact to exist a meta-nomological principle, a kind of higher law, which says: the announcement of a presumed law in science will trigger prompt and vigorous attempts at its refutation. This higher law holds in all the sciences, but it sometimes appears to enjoy its freshest expression in psychology, where, by precept and performance, we often set criticism above creation.

As regards the psychophysical power law, criticism must be left to others. My task is to explain why I think it is a law. A pleasant enough task, to be sure, but one that gives rise to a curious sense of embarrassment. Why, if the power law is so simple, obvious, and easy to confirm, did we fumble around for so long without noting its existence?

It is difficult to account for acts of unobservant oversight, for they are essentially empty and neutral. What can you say about what you did not see? One can only try to lessen the embarrassment by describing those matters that diverted attention from the proper object, or that led from seemingly sound premises to wrong conclusions.

In a larger sense, psychophysical metrics got sidetracked for a hundred years, mainly by Fechner's diligence in behalf of his famous logarithmic law. I will not dwell on that diversion, for it has been dealt with at length in another place (Stevens, 1961). To many of us who worked in audition in

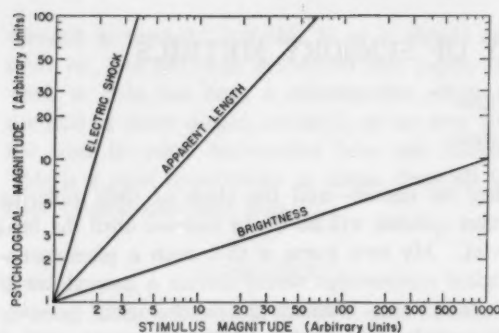


FIG. 1. In log-log coordinates the power function plots as a straight line. The exponent determines the slope. These exponents are: electric current through the fingers, 3.5; apparent length of short lines, 1.1; brightness of luminous spot, 0.33.

the 1930s there was no question that Fechner's law was awry, for did not the same scale of loudness issue from experiments that had been undertaken simply because it was so obvious to the acoustical engineers that loudness is not proportional to decibels, as Fechner's law would make it? Yes, a quarter of a century ago we knew better than to rely on Fechner's law, but, for reasons that I will try to explain, we did not know what to put in its place. All that has now changed.

EVIDENCE FOR THE POWER LAW

Within a first-order approximation, there appears to be no exception to the principle that equal stimulus *ratios* correspond to equal sensation *ratios*. (Fechner proposed, 101 years ago, that equal stimulus *ratios* correspond to equal sensation *differences*, and it is this hypothesis that the ratio rule denies and that the evidence refutes.)

The psychophysical power law relating the psychological magnitude ψ to the physical stimulus ϕ can be written

$$\psi = k(\phi - \phi_0)^n$$

where k is a constant determined by the choice of units. The exponent n varies with the modality, and also with such parameters as adaptation and contrast. Generally speaking, each modality has its characteristic exponent, ranging from about 0.33 for brightness to about 3.5 for electric shock. The value of ϕ_0 is determined by the effective "threshold" that obtains under the circumstances of the experiment. It is the point on the physical scale

from which we must start if we want to measure the effective stimulus.

The power function has the happy virtue of describing a straight line in a log-log plot—a line whose slope is determined by the exponent. How this works for three different continua is shown in Figure 1, where we see how different the slopes (exponents) can be. In linear-linear coordinates these same three functions take the forms shown in Figure 2. Slope in the log-log plot becomes curvature in the linear-linear plot.

By much trial and error—*after* it had become plain that a power function governs the growth of sensation—we learned how to get reasonably clean data from observers by asking them to estimate numerically the subjective magnitudes of a succession of stimuli. This procedure, called the method of magnitude estimation, is only one of four numerical procedures that have been elaborated, but it is perhaps the simplest to execute. In this business, simplicity and validity seem not unrelated, but it has been hard for us psychophysicists to forsake the complex, indirect procedures in favor of a plain, straightforward, direct approach.

Consider, for example, the results in Figure 3. In an effort to see how far one could go in removing all biasing constraints from the task set for the listener, I gave each listener an irregular series of loudnesses and asked only one thing: to each loudness assign the number that seems most appropriate (Stevens, 1956). That was in 1954, long before J. C. Stevens (no relation of mine) had

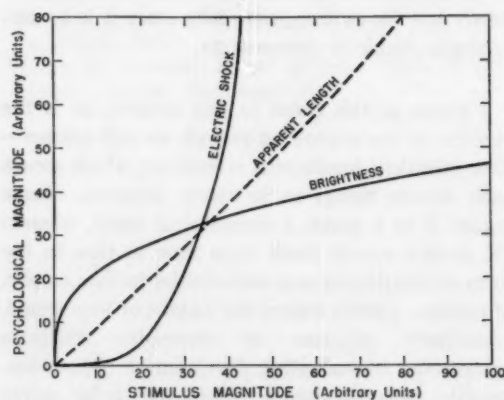


FIG. 2. The straight lines in Figure 1 become curved in linear-linear coordinates. The curvature is upward or downward, depending on whether the exponent is greater or less than one.

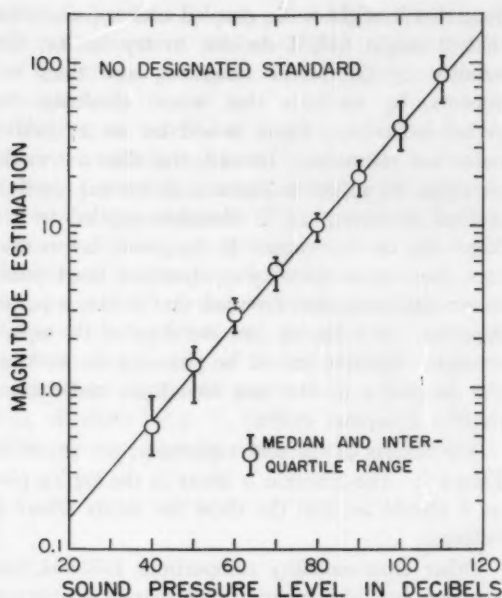


FIG. 3. Loudness function for 1,000-cycle tone. The points are the median magnitude estimations obtained when the tone was presented twice at each of eight levels in irregular order to 26 listeners. The listener assigned to the first loudness he heard any number he deemed appropriate and tried to assign proportional numbers to the succeeding loudnesses.

convinced me that the geometric mean usually gives an unbiased measure of location with data of this kind. I was still using the median, a measure that is somewhat less efficient than the geometric mean.

Incidentally, the arithmetic mean is wholly unsuitable for averaging magnitude estimations. If space permitted we could examine the plausible argument that, in all scientific work where the *relative* error tends to be constant (Weber's law), the arithmetic mean is not as appropriate as the geometric mean. In a rigorous sense, the arithmetic mean has little legitimate use in science, for its proper domain is limited to metathetic continua and the like.

The International Standards Organization has recently fixed on a function of the form shown in Figure 3 as the relation between loudness and sound pressure to be used for engineering calculations. Thus the psychophysical law is coming to have practical uses in the market place.

Meanwhile other modalities have been explored with equally good results. Figure 4 shows three

different functions determined for brightness by J. C. Stevens. Each curve was determined with a different version of the method of magnitude estimation. The observers (previously dark adapted) viewed a luminous disk of light in a dark field and made judgments of its apparent brightness. We call this the standard viewing condition (S. S. Stevens & J. C. Stevens, 1960).

Interestingly enough, the power function is little affected when the area of the target is made to fill the entire field of view. The results of three experiments by Gordon Bermant with wide-angle fields are shown in Figure 5. At the other extreme, when the target is reduced to a point source, the power function still holds, but the value of the exponent increases from about $\frac{1}{3}$ to about $\frac{1}{2}$. The squares in Figure 6 show how the line in the log-log plot grows steeper when the observer estimates the brightness of a very small target. The circles, giving an intermediate slope, are from an early

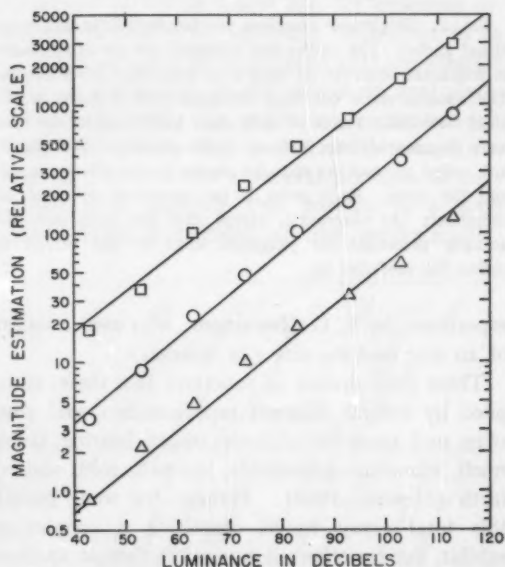


FIG. 4. Brightness functions for white light, based on geometric means of magnitude estimates made in three different experiments. *Squares*: as in Figure 3, there was no designated standard; each of the 18 observers used numbers of his own choosing. *Circles*: a standard called 10 was presented once at the beginning of each run. *Triangles*: the standard called 10 was repeated 10 seconds before each stimulus to be judged. These procedural variations made little difference to the outcome. Except where otherwise stated, the reference value for decibels of luminance is 10^{-10} lambert.

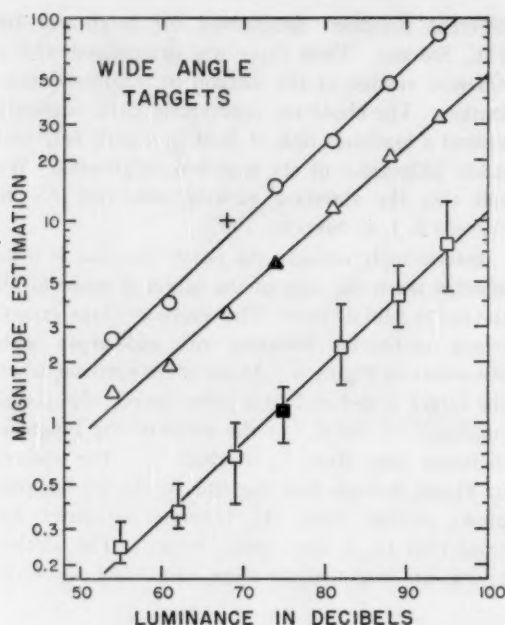


FIG. 5. Brightness functions for targets subtending large visual angles. The circles and triangles are for experiments in which the observer sat close to a large illuminated screen. The squares show the data obtained with a more nearly ideal *Ganzfeld*. Pieces of milk glass held close to the eyes were illuminated from in front of the observer. The standard, called 10, was the stimulus shown by the filled symbols and the cross. Each point is the geometric mean of 20 judgments (10 observers), except that the cross was not actually presented for judgment after it had served to define the modulus 10.

experiment by E. G. Heinemann, who used a target of an intermediate size (28 minutes).

There exist dozens of functions like these, compiled by several different experimenters, and covering such sense modalities as vision, hearing, taste, smell, vibration, kinesthesia, warmth, cold, and so forth (Stevens, 1960). Perhaps for some people this total array would constitute a convincing exhibit, but for others it is possible that no amount of this recording of verbal behavior—subjective numerical estimations—would carry conviction. Some of my esteemed colleagues make forthright objection to our treating the observer's utterances as measurements.

CROSS-MODALITY VALIDATIONS

There is nothing like a colleague's objection to send the scientist back to his apparatus. With

hope that it might work, coupled with apprehension that it might fail, I decided to try to test the validity of the power functions, and their exponents, by methods that would eliminate the verbal behavior. There would be no subjective numerical estimates. Instead, the observer would be asked to adjust a loudness in his ear until it seemed as strong as a vibration applied to his finger tip, or vice versa. If the power law is correct, these cross-modality comparisons must result in an equal-sensation function that is also a power function. In a log-log plot the slope of the equal-sensation function should be given by the ratio of the exponents of the two modalities under comparison (Stevens, 1959).

The results of the first experiment are shown in Figure 7. The function is linear in the log-log plot as it should be, and the slope lies nicely where it belongs.

Other cross-modality comparisons followed, but the huge task of comparing each sensory continuum with every other continuum has not been completed. Thus far we have had to settle for a collection of interesting samples. Much to my surprise, and that of many observers, the matching of a sound to a vibration is satisfyingly easy—certainly no harder than the task of matching two disparate

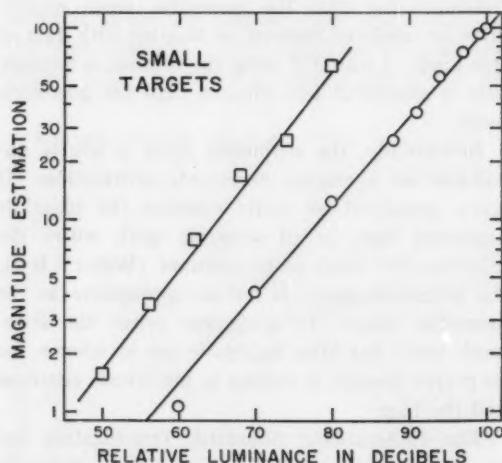


FIG. 6. Brightness functions for small targets subtending visual angles of 1.5 minutes of arc (squares) and 28 minutes (circles). The abscissa gives relative levels only. The highest level for the smaller target was about 110 db re 10^{-10} lambert. The larger target had a different highest value in each of four different experiments, all of which were averaged to obtain the circles.

sounds in loudness, or two colored lights in brightness. Furthermore, the variability is reasonably well behaved, so that geometric averaging is clearly appropriate.

The largest number of cross-modality comparisons made thus far have been with handgrip. Instead of vocalizing, the observer merely squeezes a precision dynamometer (Fig. 8) to indicate his judgment of the apparent strength of the sound, or the light, or the electric shock. First we must ask what happens when the experimenter names numbers in irregular order and the observer squeezes what he judges to be a proportional amount (method of magnitude production). Typical results, obtained by J. C. Stevens and J. D. Mack (1959) in two separate experiments, are shown in

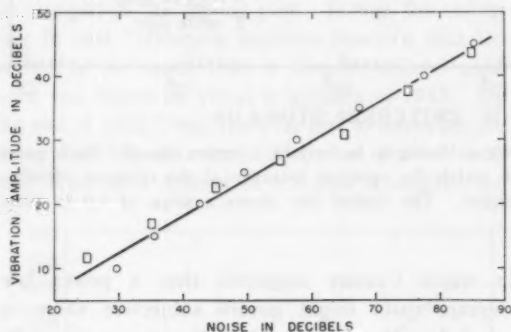


FIG. 7. Equal-sensation function produced by the cross-modality matching of loudness to vibration (60 cps on fingertip). *Circles*: the loudness was adjusted to match the vibration. *Squares*: the vibration was adjusted to match the loudness. Each procedure determines a slightly different slope, but the two experiments together define a power function whose exponent is equal to the ratio between the exponents for loudness and vibration.

Figure 9. Each line is for a different observer. Presented in this manner, the data show that each observer follows a power function when he judges apparent force of handgrip. From this and other studies it appears that, for the median observer, the sensation of strain in the production of handgrip grows as the 1.7 power of the force exerted. When a person actually squeezes twice as hard, he judges it to be about three times as hard.

Equipped with this dynamometer and the measured exponent for force of handgrip, we have gauged the growth of sensation on nine other continua by asking observers to emit squeezes instead of numbers. The resulting equal-sensation func-

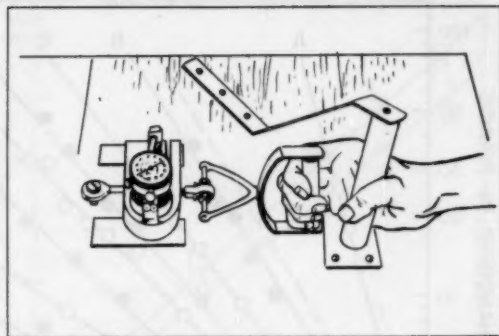


FIG. 8. One of the hand dynamometers.

tions are shown in Figure 10. When the exponents determined by the slopes of these functions are multiplied by the factor 1.7, the resulting values agree remarkably well with the exponents measured directly by magnitude estimation (Stevens, Mack, & Stevens, 1960).

The largest discrepancy between the exponent determined by verbal report and the exponent determined indirectly by squeezing turned out to be 0.07. We regard it as rather good that the greatest difference occurs only in the second decimal place.

Confronted with this richly interconnected evidence, some of us find it difficult to escape the belief that there exists a general principle of psychophysics—a principle that governs to a good approximation throughout all the prosthetic per-

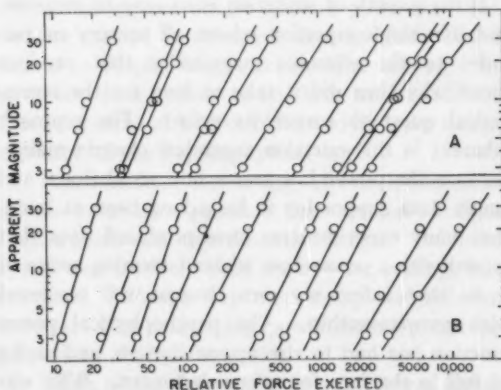


FIG. 9. Functions for apparent force of handgrip obtained by magnitude production. The experimenter designated certain numerical values (ordinate) in irregular order, and the observer produced proportionate squeezes (abscissa). Each curve is for a single observer. Plots A and B are for two different kinds of dynamometer. Points are medians of 7 squeezes (A) and 10 squeezes (B).

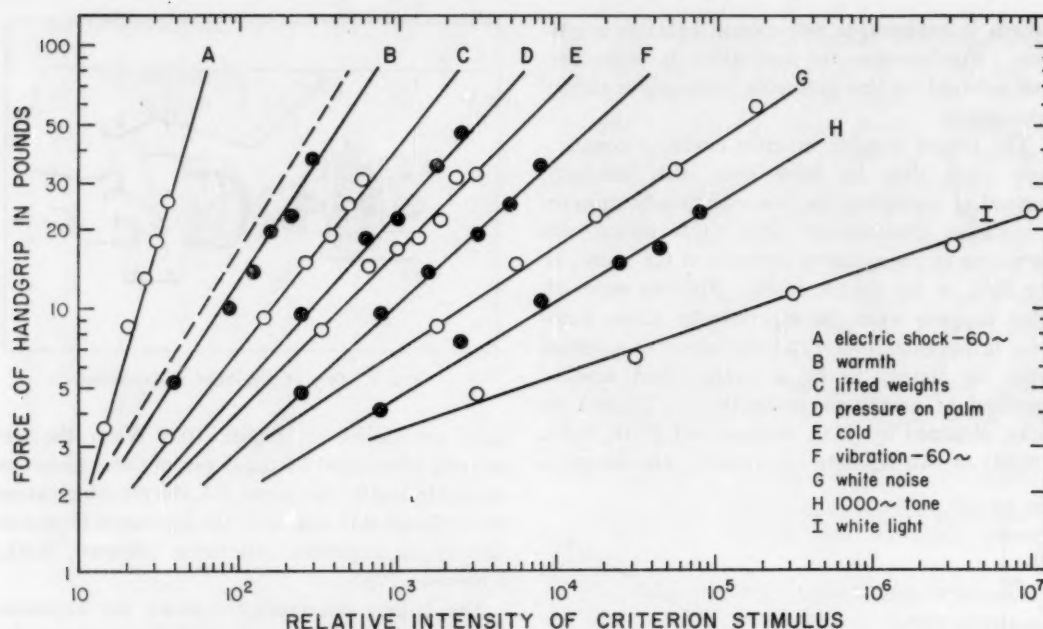


FIG. 10. Equal-sensation functions obtained by matching force of handgrip to various criterion stimuli. Each point stands for the median force exerted by 10 or more observers to match the apparent intensity of the criterion stimulus. The relative position of a function along the abscissa is arbitrary. The dashed line shows a slope of 1.0 in these coordinates.

ceptual domain. Psychophysics, we venture to suggest, has found itself a law.

DIFFICULTIES AND IMPEDIMENTS

If the growth of sensation is so easy to measure, and if a single equation relates all sensory magnitudes to the stimulus magnitudes that produce them, why then did it take so long for the nomological quest to corner its prize? The reproach inherent in this question engenders chagrin mainly because the power law seems now so obvious, and ample data supporting it have long been at hand. But many experimenters have produced data that accord with a power law, without seeming aware of it, so that I find my own chagrin well tempered with companionship. The psychophysical power function has had to rise up, as it were, and strike us full in the face for acknowledgment. Why was this so?

First I should hasten to note that nothing is ever without its antecedents, and like certain other functions the power function has had its occasional champion. It begins perhaps with a letter by Gabriel Cramer, cited by Daniel Bernoulli (1738),

in which Cramer suggested that a power law (square root) might govern subjective value—a psychological variable that the economist calls *utility*. Bernoulli favored a logarithmic function, the same function advocated a century later by Fechner. Some time in the 1850s Plateau seems to have proposed a cube-root law for apparent brightness, or at least Delboeuf says he did, but Plateau later changed his mind about the power law. He defected, as it were, when he was confronted with some of Delboeuf's data. Then there were various theories regarding differential sensitivity, two of which, Brentano's and Guilford's, took forms that led, via different sets of questionable assumptions, to a power law. So we see that the power function was certainly not unheard of (Stevens, 1957). Indeed, it was like many other mathematical functions that are constantly being tried out here and there. For instance, the power function was one of three equations tried out in 1932 by Ham and Parkinson when they were looking for a formula to fit their results on loudness estimation.

Ham and Parkinson were on the right track, it

now appears, but the *piste* promptly got itself obscured by an unfortunate experiment on loudness fractionation by Laird, Taylor, and Wille (1932) whose results have ever since eluded explanation and repetition. Stevens, Rogers, and Herrnstein (1955) even tried to repeat them with the aid of the same kind of antique audiometer. But other experimenters continued to worry about the rather obvious fact that loudness does not seem to grow proportional to decibels (as Fechner's logarithmic law says it should), and experiments continued to accumulate.

In 1938 Davis and I were able to publish the loudness function for the standard 1000-cycle tone in the form shown in Figure 11. There we see an empirical function based on considerable data, but plainly it is not a power function, for it does not lie straight in the log-log plot. It was the curvature in that 1000-cycle loudness function that led us astray for more than a decade—indeed until work was begun on visual brightness in 1953. By the end of 1953 I was ready at last to advocate the power function (Stevens, 1954), but I would still have been hard pressed to convince a skeptic, for

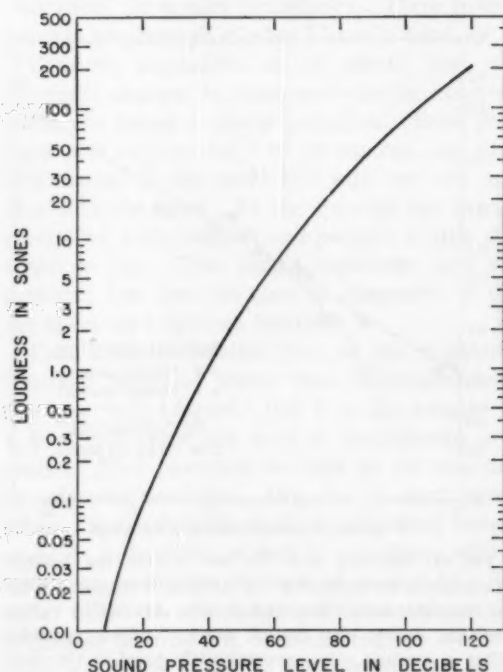


FIG. 11. An early form of the loudness function for the 1,000-cycle tone. The curvature may be ascribed to three factors, none of which was suspected in 1938.

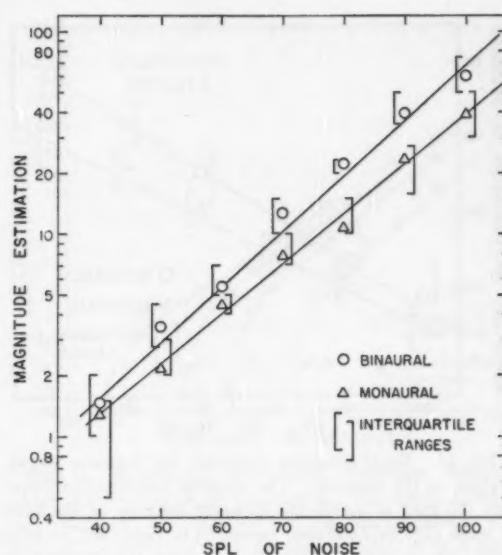


FIG. 12. Loudness functions for binaural and monaural listening. The signal was a band of noise 250 to 2,000 cps. Levels were presented at random to right, left, or both ears and the observer estimated the apparent loudness.

the evidence was not overwhelming. Not yet. But then began a long, ebullient period of proving and testing the law on a score of continua, and by every means we have been able to devise.

REASONS FOR CURVATURE IN THE LOUDNESS FUNCTION

Why did we not obtain a clean power function when we plotted the loudness data in 1938? If the truth is so simple, how could we have missed? In retrospect there appear to be three causes. Three different sources of bias combined to hide the naked simplicity of the psychophysical law.

1. Fractionation, the setting of one loudness to half the value of a standard, has the built-in bias of an unbalanced experimental design. In those days it was a widely used method for scaling, but sometimes *halving* was not complemented by *doubling*. The underlying power function can be obscured by the resulting bias.

2. Much of the data available in 1938 was based on binaural vs. monaural loudness balances. This was because Fletcher and Munson, two important pioneers in loudness measurement, had made the assumption that a sound in two ears is twice as loud as the same sound heard in only one ear. Furthermore this simple and engaging rule seemed to be

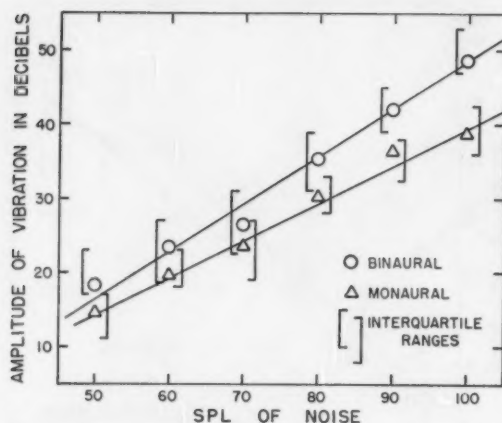


FIG. 13. Equal-sensation functions for loudness vs. vibration on the fingertip. The observer adjusted the vibration (60 cps) to match the apparent loudness of the band of noise (250 to 2,000 cps) presented to right, left, or both ears. Cross-modality matching confirms the evidence in Figure 12 that monaural loudness grows less rapidly than binaural loudness.

nicely confirmed by fractionation experiments. That, indeed, is the unfortunate aspect of it: the assumption of perfect binaural summation turns out to be *almost* correct. It is, in fact, so nearly correct that it required a many-pronged attack to prove that the rule is false (Reynolds & Stevens, 1960). Samples of some of this work are shown in Figures 12 and 13. These and many other observations have shown that binaural loudness grows with an exponent of 0.6, whereas monaural loudness grows with an exponent of 0.54. At one sound pressure level (90 db) the binaural loudness happens to be exactly double the monaural loudness, but at no other level does this simple ratio appear to hold. The false assumption that the 2-to-1 ratio holds throughout the scale was a factor that helped put curvature into the loudness function, which caused us to look for a complicated equation when a simple one would do.

3. Even when all biases in the procedures and all false assumptions regarding the binaural-monaural relation have been cleared away, there still remains some residual curvature near the lower end of the loudness function. That is the reason for the constant φ_0 in the equation above. In order to get a power function one must measure the stimulus beginning at threshold, not at the conventional zero of the physical scale. This need for a threshold correction was most dramatically evi-

dent in our work on the perception of warmth and cold, because absolute zero on the temperature scale is far removed from "physiological" zero (J. C. Stevens & S. S. Stevens, 1960). With the temperature senses it would seem silly to measure from absolute zero—and indeed it is.

With sufficient care one can also show the need for φ_0 in the loudness equation, even though its value is small. A careful exploration of the low end of the loudness function by B. Scharf and J. C. Stevens (1961) gave the data in Figure 14. The unfilled points that curve downward are the uncorrected values. The subtraction of a constant φ_0 from each of the experimental points makes them all lie reasonably close to the same straight line and thereby restores the power function.

It was against the foregoing three sources of obfuscation that the power law had to fight its way into clarity. Curiously enough, it was only by working on other sense modalities that we found out what was wrong with the early form of the loudness function. If knocking your head against one wall produces no answers, it sometimes pays to knock against another wall.

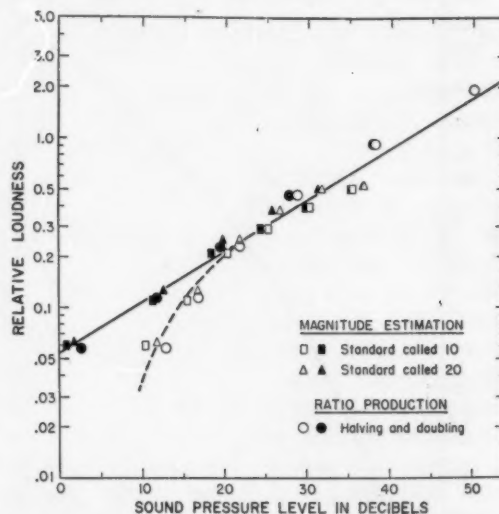


FIG. 14. Showing how the low end of the loudness function can be rectified if the stimulus is measured starting from threshold. The original data, obtained by various methods, follow the dashed curve (unfilled symbols). When a constant value, approximately equal to threshold, is subtracted from each experimental value, the filled points are obtained. The corrected points fall close to the same function, represented by the straight line with a slope of 0.6.

PERTURBATIONS

I do not mean to imply by the foregoing remarks that loudness can now be shown to follow a perfect power function. It is probably not quite that simple—at least not for sounds of all spectra (Stevens, 1955). The scientific leverage that accrues from having an equation adequate to the first-order sensory transductions is simply this: once the first-order effect is reduced to a formula, the second-order departures from the basic law may conceivably lead to new and deeper understanding.

One is reminded, for example, that the discovery of the planet Neptune resulted from the stubborn refusal of the planet Uranus to follow precisely the law of the heavens, as ordained by Newton. Do perturbations in the power law foretell the discovery of new factors in the sensory process? That question sets a task for the future.

PARAMETRIC EXPLORATIONS

Another task for the future has already been begun. It calls for the enlargement of the psychophysical law to embrace the principal parameters that affect the sensory transducers. There is time for only one example of what I have in mind.

Consider adaptation as it affects the eye. Dramatic changes in brightness can be observed when you follow a simple procedure. Hold your hand over one eye for 5 to 10 minutes, and then look around at the world first with one eye, and then with the other. To the eye that was closed, everything looks brighter and perhaps a little different in hue. That simple experiment sets the problem: how does the state of adaptation of the eye affect the brightness function?

First I should mention that all the brightness functions described above were determined with the eyes "dark adapted," that is to say, adapted to a level well below the level of the stimulus presented. That procedure we take as the standard or reference condition. One has to start somewhere. In the dark-adapted state the visual system responds to luminous stimuli in accordance with a power law having an exponent of about 0.33. We define a *bril*, the unit of the brightness scale, as the brightness seen by the dark-adapted eye when it views a 5-degree target at a luminance that is 40 decibels above the reference level 10^{-10} lambert.

Our next concern is to measure the effect of changing the state of the eye by adapting it to a

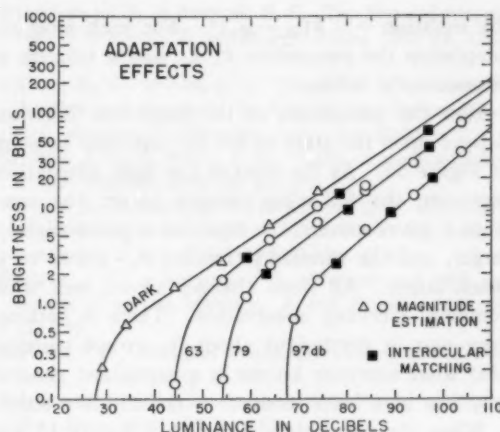


FIG. 15. Results of determining brightness functions under different states of adaptation. The upper curve is the "standard" bril function for the "dark-adapted" eye. Each of the other curves is for the eye adapted to a given luminance, indicated in decibels re 10^{-10} lambert.

prescribed level of luminance. In order to measure these effects we take advantage of the fact that we have two eyes: we light adapt one eye and dark adapt the other. With the two eyes thus differently adapted, we can do two kinds of experiments. We can measure the brightness function for each eye separately by the method of magnitude estimation. We can also compare these two functions directly by *matching* the brightness seen by the dark-adapted eye to that seen by the light-adapted eye. This interocular matching procedure has been used by Hering and many other workers.

Results of both procedures, matching and magnitude estimation, are shown in Figure 15. Each curve is for a different level of adaptation. In each of the many experiments (carried out mainly by J. C. Stevens), the left eye was dark adapted and the right eye was light adapted. (The right eye stared at a large white cardboard illuminated at various levels.) When the eyes were fully and differently adapted, the test target, subtending about 5.7 degrees, was presented briefly to one eye or the other.

The experimental data in Figure 15 show two things. Under different levels of adaptation, the "operating characteristic" of the visual system continues to be a power function, but both the "operating point" and the "gain function" are affected by light adaptation. Stated in another language, light adaptation alters each of the parameters in

the equation $\psi = k(\varphi - \varphi_0)^n$. For each level of adaptation the parameters k , φ_0 , and n take on a characteristic value.

How the parameters of the brightness function depend upon the state of the eye can best be seen in Figure 16. As the level of the light adaptation increases, the following changes occur: the constant k grows smaller, the exponent n grows slightly larger, and the threshold (labeled B_0) grows very much larger. All these changes accord well with common, everyday observation. There is nothing very new or mysterious about it—except perhaps that what everyone knows in a vague and general way has here been reduced to quantitative order.

When the parameters depicted in Figure 16 are used to generate a complete family of brightness functions, like those shown in Figure 17, a basis is laid for a new and fuller understanding of the visual transducer. These functions tell us how any given target, exposed for a second or two, will look to an eye in any of several states of adaptation (S. S. Stevens & J. C. Stevens, 1960).

The implications of these functions could be spelled out in a long chapter, but we have time for only one of the more interesting details. A series of circles appears in Figure 17 connected by a dashed line. These circles mark the locus of the equilibrium function—the brightness seen when the eye, adapted to a given level, is shown a stimulus at that same level. Another name we give the dashed curve is the "terminal brightness function," because

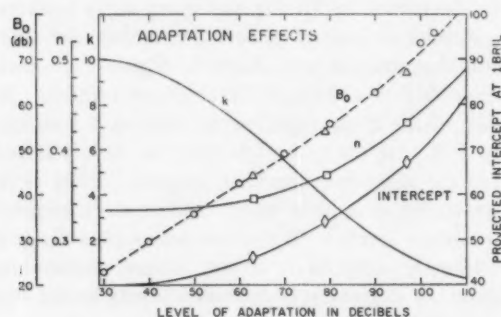


FIG. 16. Parameters of the brightness equation as a function of level of adaptation of the eye. The exponent values (*squares*) were read from Figure 15, as were the 1-bril intercepts (*diamonds*). The values of k were then calculated on the assumption that the luminance B is measured in millilamberts. The threshold values of B_0 were estimated from Figure 15 and plotted as triangles. The circles show the threshold values directly measured by P. G. Nutting.

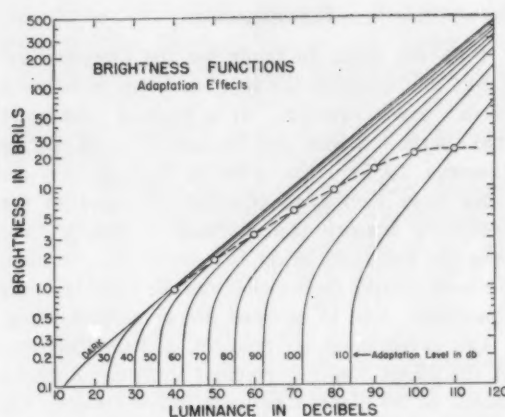


FIG. 17. Family of brightness functions for a wide range of adaptation levels. The dashed line is the terminal brightness locus—the level of sensation reached when the eye comes into full equilibrium with the luminance it is viewing.

it is the function that tells us how bright a target will look after the observer has stared at it for a long period of time—long enough to reach full adaptation. This terminal brightness function does not follow a power law.

We note another interesting feature: the dashed curve becomes horizontal at the upper end. If the viewer becomes fully adapted to the level of the stimulus, does he then see a maximal brightness regardless of the stimulus intensity? The answer is yes. This remarkable prediction accords with a result by K. J. W. Craik (1940), who pursued this question to the heroic level of 75,000 foot-lamberts (119 db re 10^{-10} lambert). Levels like that are about ten times greater than the luminance of snow under the noonday sun. The equilibrium brightness function measured by Craik was still flat at the high level.

Much more could be said, and no doubt will be said, about the varied and exciting principles that regulate the input-output functions of the sensory transducers. Tortuous and delayed as may have been the discovery of the basic law—the psychophysical power function—the way is now clear, even for the potential usefulness of proven discrepancies, when such there are.

One final note. The power function has asserted itself not only on continua that involve well-known stimulus variables, but also on a continuum, tactual roughness, for which we had at first thought

there would be no metric stimulus-correlate (Stevens & Harris). Our first guess proved delightfully in error, for we found that apparent roughness grows as the 1.5 power of the diameter of the abrasive particles on standard emery cloths. When the observers judged the apparent smoothness of the same emery cloths, the exponent turned out to be nearly equal in magnitude, but opposite in sign. We thereby demonstrate the observer's remarkable ability to judge a continuum in terms of its reciprocal function. Judging the inverse aspect is not as easy, it seems, but people do remarkably well at it. Furthermore, reciprocal functions were produced rather exactly in two cross-modality experiments in which ten observers matched loudness to roughness, and ten others matched loudness to smoothness. Here again the measured exponents were equal in magnitude but opposite in sign. The testimony of these reciprocal judgments adds another dimension to the network of evidence supporting the power law.

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NEW MEMBERS AND ASSOCIATES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

THE Board of Directors announces that the following 1,277 persons were elected to membership in the American Psychological Association as of *January 1, 1962*. Of these, 490 were elected as Members, and 787 were elected as Associates. Though not all of them have validated their election by payment of dues, all but a few eventually will do so. In accordance with Article II, Section 8, of the Bylaws, 178 Associate members, having obtained their doctoral degrees in psychology, have been transferred to Member status as of *January 1, 1962*.

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Johnson, Thomas J.
Johnson, Yvonne Elizabeth
Jones, LeRoy Alphonso
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Jordan, Augustus Eugene
Jorgensen, Ronald Leland
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Kadushin, Phineas
Kagerer, Rudolph Lucas
Kahn, Dale
Kahn, Rosalind Jablon
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Kallos, George Louis
Karp, Eric
Karraker, Mary Elizabeth
Katahn, Martin
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Keefe, Sister Mary Karen
Kehrberg, Willard Everett
Keiter, Dorothy Henne
Kelson, Florence
Kemp, David Edward

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 Kempson, J. Obert
 Kennedy, Daniel Arnold
 Kephart, Lester Merlin
 Ketterling, Marvin Earl
 Kieferle, Dawn A.
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 Killeen, Carolyn Mae
 King, William Judson
 Kish, Gerald R.
 Klein, Stanley David
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 Koban, Nuria Cortada
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 Kreul, Randolph W., Jr.
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 Lane, James Albert
 Larschan, Edward Joseph
 Larson, Cedric Arthur
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 Laughlin, Patrick Ray
 Lawson, John Richard
 League, Betty Jo
 Lee, Lee C.
 Lefcourt, Herbert Michael
 Lenard, Henry Madart, Jr.
 Leon, Joy Ann
 Leonard, Calista Verne
 Lerman, Hannah
 Lesquin, Elizabeth Skeehan
 Lesser, Alan Martin
 Levin, Seymour
 Levine, Florence Spivak
 Levine, Sara
 Levitov, Edith Sylvia
 Lim, Betty Y.
 Lindsay, Carl Albert
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 Llewellyn, Lynn Gresham
 Logan, James Clifford
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 Lorenzo, Leo Arthur
 Losak, John George
 Louis, Eugene
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 Loveland, Ruth Culp

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 Luchsing, Vincent Peter, Jr.
 Lunneborg, Patricia Wells
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 Mabry, John Homer
 MacLachlan, Robert Francis, Jr.
 Malinowski, Frank Andrew
 Maliver, Bruce Lawrence
 Mallory, C. Eugene
 Manning, Stephanie Ann
 Marans, Mary Rosenberg
 Marker, Catherine Stewart
 Maroldo, Georgette Kathleen
 Marshman, Cameron Stanley
 Marsico, William Edward
 Martin, Ann Mildred
 Martin, Dikran John
 Marvin, John Bingham
 Matheny, Kenneth Buren
 Mather, Leonard Joseph
 Mathews, Dewey L., Jr.
 Matthews, Floy Wetzel
 Mayfield, Peter Nelson
 Mazrim, Frank Russell
 McBee, Mary Louise
 McCarthy, Wilbur Don
 McConvey, Margaret Monica
 McCormick, Ronald Robert
 McDonald, Robert Drury
 McGinnis, Charles Alan
 McGowan, Barbara Ellis
 McGuigan, Derrill I.
 McIntire, Roger Warren
 McKeithen, Eleanor Jean
 McLain, Richard Edward
 McLaurin, William Arthur
 McLean, Joel Albert
 McWilliams, George Norman
 Meers, Josephine Marie
 Meidinger, Thomas Arthur
 Mercer, Robert
 Meyer, Marv L.
 Mial, Harry
 Middleton, Jane Ethelyn
 Miller, David Louis
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 Mogin, Lenore S.
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 Moll, Arthur Ingrahm
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 Moore, Betty Eileen
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 Mowry, Frederick Steele
 Moyel, Isaiah S.
 Munro, Clare
 Murphy, Genevieve M.
 Murray, Joseph Edward
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 Nash, Ralph Julius
 Needham, Walter Evans
 Nelson, Laura W.
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 Nelson, Ted
 Nesvan, Geraldine A.
 Neuhaus, Maury
 Niebuhr, Miriam
 Nobers, Donald Richard
 Nolan, Edward Peter
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 Norriss, Norma G.
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 Palmer, Albert B., Jr.
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 Payette, Pierre Roger
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 Penick, Elizabeth Carnel
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 Powell, Lillian Ruth
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 Quiriconi, Roy Jordan

 Rachman, Arnold
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 Rapoport, Benzion Jacob
 Ravnitzky, Gerald Philip
 Reardon, Beverly Weisgerber
 Reed, Roland R.
 Reed, Walter Graham
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 Riffel, Pius Anthony
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 Tauber, Ronald Keith
 Terhune, Kenneth Warren
 Thackeray, Selma L.
 Theiner, Eric Charles
 Thomas, Mary Magdalene
 Thomas, Paul Leslie
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 Thompson, Dorothy Moss-
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 Thompson, Richard War-
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 Thor, Donald Harold
 Tobin, Sheldon Sidney
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 Turoff, Bernice Basser
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Comment

On Boards of Examiners in Hypnosis

After reading in the April 1961 *American Psychologist* the account of the tripartite American Board of Clinical Hypnosis and its certification of physicians, dentists, and psychologists through three sub-boards, we consider it our duty to call attention to the fact that there exists a strong body of opinion which is opposed to the formation of such examining and certifying boards. No one reading this article alone would suspect that such a difference of opinion exists, nor would he be aware of the existence of an organization for the study of hypnosis which is entirely unconcerned with examining boards—the American Society of Clinical Hypnosis, founded in 1957 and having 2184 member in the professions of medicine, dentistry, and psychology.

The principal reasons for opposition to these boards, which are set out in an editorial in the *American Journal of Clinical Hypnosis* for July 1960, are:

1. Hypnosis is not a specialty but a technique which can be used by different professions and in various specialties within these professions. The APA's Board of Professional Affairs knew this fact, since it reported that it "initially expressed concern over the certification of a technique as such. . . ." The fact that the American Board of Examiners in Psychological Hypnosis referred to the three boards as "these three specialty boards" shows how hard it is to eliminate confusion on this matter, even though in their immediately preceding sentence they had stated that "Hypnosis is not an independent discipline."

2. It is very doubtful that the establishment of such boards can achieve one of the most important goals of specialty boards, that of protecting and informing the public, since there are lay organizations which issue certificates in hypnosis. The public can hardly be expected to discriminate among the various diplomas which are available, and it is regrettable that a professionally unqualified person can display credentials which are in the eyes of the public equal to, if not better than, those of the legitimate practitioner.

3. The existence of affiliated boards in three professions sub¹ implies that there are different kinds of hypnosis, each requiring a separate certification. A comparable situation would be separate certifications in anesthesiology for orthopedic and for abdominal surgery. They also imply that the hypnosis itself is the major consideration rather than the field of professional competence in which the hypnosis is used.

Some other comments: The requirement for the

Diploma of Clinical Hypnosis of "agreement to adhere to the published Code of Ethics of APA . . ." seems superfluous, since all APA members should consider themselves bound by the fact of their membership to follow the Code. The Diploma in Experimental Hypnosis, which requires "significant research publication in the field of hypnosis and five years of acceptable experience in experimental hypnosis," is at least unique, if that be a virtue. There is no other field in experimental psychology that we know of in which any similar certificate is considered to be desirable.

FRANK A. PATTIE
MILTON H. ERICKSON
University of Kentucky

Feedback Study of Psychology Graduates

The article "Feedback from bachelor of arts psychology graduates" by W. Leslie Barnette, Jr. (*Amer. Psychologist*, 1961, 16, 184-188) shows some interesting parallels to a similar study conducted at Southern Illinois University at about the same time. All but one of the BA psychology graduates were sent a questionnaire regarding their retrospective evaluation of our undergraduate psychology program. With one follow-up letter, an 80% return was obtained. Of these, 55% had taken or were taking graduate work in psychology, while the remainder were mostly distributed in professional, sales and managerial, and military positions. Would they major in psychology again? Eighty per cent said they would; the same proportion said they would so advise a young friend "whose background and interest were similar" to theirs. Two-thirds of those not having gone on to graduate work reported they make use of their psychology training in their occupations; only one-third felt their psychological training helped in getting their present job. The main value for them appeared to lie in the "understanding" they feel they got from majoring in psychology. Three-fourths of those having graduate work in psychology regarded their undergraduate preparation for graduate work as either "excellent" or "adequate." However, suggestions for the improvement of courses and curricula revealed some agreement on "weak spots." By and large, students having graduate work requested more emphasis be given to experimental, physiological, statistical, and similar basic topics. Not surprisingly, students not continuing in psychology asked for more emphasis on freedom of choice and the "application of facts to real life."

Aside from (hopefully) some positive reinforcement for one's labors, such surveys provide a department with one potentially valuable opportunity: the chance to include student feedback as one source of information in curriculum revision. This was done in our case. We all must wonder, at least occasionally, how our efforts contribute to our students' later activities. This is one way of finding out.

NEIL A. CARRIER
Southern Illinois University

Illness, Responsibility and Intervention

In a recent comment (*Amer. Psychologist*, 1961, 16, 601) Rothstein attributes a view to me which I explicitly repudiate in my article, "Personality disorder is disease" (*Amer. Psychologist*, 1961, 16, 69-74). Rothstein writes: "Ausubel contends that most mentally ill patients should be considered to be victims of a disease process and accorded the same exemption from ethical considerations as is enjoyed by people with any illness."

It is quite clear, however, that I said precisely the opposite of what Rothstein attributes to me. I insisted that

... it is possible in most instances ... to distinguish quite unambiguously between mental illness and ordinary cases of immorality. The vast majority of persons who are guilty of moral lapses knowingly violate their own ethical precepts for expedient reasons—despite being volitionally capable at the time, both of choosing the more moral alternative and of exercising the necessary inhibitory control ... *They are not mentally ill, but they are clearly accountable for their misconduct.* ... In those instances where warranted guilt feelings do contribute to personality disorder, the patient is accountable for the misdeeds underlying his guilt, but is hardly responsible for the symptoms brought on by the guilt feelings. ... Hence, since personality disorder and immorality are neither coextensive nor mutually exclusive conditions, the concept of mental illness need not necessarily obscure the issue of moral accountability.

I fail to see how this statement can be construed as a plea to exempt anyone, mentally ill or otherwise, from accountability for moral lapses reasonably within his control.

In Tolor's comment in the same issue, he misses the main point of my article when he claims that I justify considering personality disorder a disease on the grounds that "since medical intervention may affect the psychological behavior, the latter falls within the realm of a disease process" (p. 601). The actual criterion of disease which I propose in my paper is "any marked deviation, physical, mental, or behavioral, from normally desirable standards of structural and functional integrity" (p. 71). I take the position that

all diseases are *not* medical and need not necessarily be treated by medical men or with medical measures (p. 70). A serious learning disability would be a good example of a nonmedical disease.

It is true that I justify the use of medical measures in psychogenic disorders on the grounds that manipulation of the "neural substrate *can conceivably* have therapeutic effects" (p. 72). But this hardly means that I advocate medical measures in *all* cases. Nor does it mean that the possible efficacy of such measures is the criterion I use in referring to personality disorders as a disease. It simply means that it is just as rational to treat psychogenic depression with electroshock therapy as it is to administer antibiotics in cases of general paresis, i.e., that the psychogenicity of a personality disorder does not necessarily rule out medical treatment. This position states nothing new. It is at least as old as the rationale for insulin shock therapy.

DAVID P. AUSUBEL
Bureau of Educational Research
University of Illinois

On Training in Engineering Psychology

In the April 1961 issue of the *American Psychologist*, the Division 21 Committee on Training in Engineering Psychology presented a report covering the scientific and professional aspects of human factors engineering, with special emphasis on training programs intended to provide adequate preparation for specialization in this area. While the underlying philosophy and specific training programs outlined in the article are, in general, satisfactory a few points need to be emphasized and a few critical remarks need to be made.

One of the major criticisms of the article is the lack of a more definitive program of study at each educational level up to and including the PhD degree. Undoubtedly, this nonspecificity was intended to provide flexibility in training. However, the reader is likely to be left with the impression that "three special courses ... which might ... be added to the more traditional offerings" are all that is required to produce an engineering psychologist. Although the article does indicate that "the graduate student who elects to major in engineering psychology should be advised to take extensive course sequences outside of psychology," too little stress is placed on the interdisciplinary nature of human engineering. Furthermore, not enough attention is directed to the need for professional experience which should be gained either in the laboratory or on the job while the student is actively pursuing his degree requirements.

Considerable space is devoted in the article to a detailed description of the various technical and professional activities in which engineering psychologists

are currently engaged. Such an exposition may be helpful to the nonengineering psychologist, but the article fails to bridge the gap between job assignments and requisite academic training. There is no direct correspondence between the professional and scientific activities described and the training programs recommended. The implication inherent in the article seems to be that any individual trained to a certain academic level should be able to perform *any* human factors assignment commensurate with that level of training. This is not realistic; even within engineering psychology there are subspecialties. Also, the analysis of the activities in which engineering psychologists are currently engaged does not answer the question regarding the kinds of activities in which they *should* be engaged. For example, there are certain operational suitability tests and evaluations which are conducted on prototype items and systems which are *clearly* the responsibility of engineers, not psychologists. Programs of study in engineering psychology should be developed from the broader objectives of psychology as a science and as a profession, and not from the deficiencies, e.g., in experimental methodology, which may be found in other disciplines.

The Committee is to be commended on its attempt to state in a more or less formal manner the training requirements for engineering psychology. The report provides an excellent summary of many existing practices in educational institutions, in industry, and in governmental organizations. It is hoped that at some future time the Committee will address itself to the task of prescribing course programs which are not only more specific, but which will serve to guide the continued growth of engineering psychology instead of merely reflecting the past course of events.

JOHN F. CORSO
Pennsylvania State University

A Prince is Not a Prince

It is probable that many psychologists will have shared my surprise, mingled perhaps with a sense of pride, upon reading in Jones' (1957, p. 287) biography of Freud that Morton Prince had once been mayor of Boston:

In the middle of January we heard there had been a furor in Boston. The police there, no doubt with some instigation, had threatened to prosecute Morton Prince for the 'obscenities' he was publishing in his *Journal of Abnormal Psychology*. So his generosity to psychoanalysts was ill rewarded, and there was some justification for his misgivings which Freud had wrongly attributed to his 'puritanical prudishness.' But Prince, who had not long before been Mayor of the city, knew how to weather such storms without having to appear in court.

A bit of sleuthing, however, disenchanted me of the ambrosial sensation that a fellow psychologist had once occupied such an exalted public office. To set the record straight, I must report, though reluctantly, that it was not Prince, Morton, but rather his father, Prince, Frederick Octavius, who had been mayor of Boston, having been elected in 1876, and holding "... the office for four successive terms, with the exception of one year." (*The national cyclopaedia*, 1909, p. 222.)

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ROBERT PERLOFF
Purdue University

Psychology in Action

PSYCHOLOGY IN NATIONAL AND INTERNATIONAL AFFAIRS: A PROGRESS REPORT

FOR slightly over two years, the APA has sustained a program in national and international affairs, first on an exploratory basis as a Working Group under Roger W. Russell's direction in 1959-60, and now, since the Fall of 1960, as a standing Committee led by Charles E. Osgood. To date, Russell's report to the APA Board of Directors in 1959, "Roles for psychologists in the maintenance of peace" (*Amer. Psychologist* 1960, 15, 95-109), has been the principal public document on the shape and direction that such a program might take. The present report is intended to fill the gap with a brief coverage of the events leading to the establishment of the Association's standing Committee, and a review of its activity since that time.

In creating the Committee, the Board directed that it be concerned with: (a) examining international and related national issues for those aspects to which the special competencies of psychologists may be applied; (b) initiating and coordinating actions, approved by the Board and Council, which are likely to encourage such applications; (c) maintaining liaison with APA boards, committees and divisions, and with other individuals and organizations whose special interests intersect those of the Committee.

A great deal of prior consideration had foreshadowed this development. In 1957, as APA's Executive Secretary, Roger Russell began an investigation into ways psychology might contribute to matters of national policy, especially in relieving problems of international tension; this involved a solicitation of written comments from many segments of the Association membership, and culminated in his report. In response to this initiative, the Board named a Working Group in 1959, consisting of Paul Fitts, Harold Guetzkow, E. P. Hollander, Daniel Katz, C. E. Osgood, M. Brewster Smith, Joseph Weitz, with Roger Russell as Chairman, to study and report after one year on the feasibility of mounting an APA program in this direction.

The Working Group carried forth an investigation along several lines: an examination of the state of activity and knowledge within other organizations and groups interested in similar problems, a review of relevant research undertakings completed or contemplated, and an interview study among psychologists and other behavioral scientists in or near government circles in Washington. The findings of this investiga-

tion provided an outline of current realities and guidelines for action. The Working Group, in reporting favorably for an APA program, said in part: "We are not smug in the belief that psychologists alone are competent to solve these issues, but we do believe that a strong case has been made in the 1959 report [Russell, *supra*] and in our present report for support by APA of an organized effort to make what appear on a priori grounds to be potentially useful contributions."

Some of the more prominent elements in this 1960 Working Group report came from the interview study exploring the roles psychologists were playing in the development of public policy, as well as the outlook there for the future. More than two dozen observers had been contacted. Their comments signified, for the most part, that no acknowledged role for psychology yet existed in governmental policy determination. In certain areas, e.g., education and training, personnel selection, human engineering, it was said that applications from psychology had gained substantial acceptance; then, too, there was reference to a generalized absorption of psychology into the culture, with varying degrees of understanding. At the level of governmental departments, however, psychological knowledge, from psychologists, apparently was not a significant input to policies and programs.

In these observations, a distinction was often evident between the methodology of psychology, in identifying variables, measuring behavior, and employing refined statistical treatment and the accumulated knowledge of psychology in findings and theory. Operationally, this held the implication that the psychologist might be equipped to do research on a problem but not advise on it from a body of established principles—a form of "underselling" that seemed to be a response to the alleged "overselling" of psychology that a number of these respondents viewed with considerable disfavor. Counterbalancing such concerns was the very real evidence on every hand that psychology and the behavioral sciences were increasingly coming into their own in governmental agencies supporting research, and that this would soon have ramifications in various quarters of government. Whether by design or fortuitous circumstance, the conclusion was reached that psychologists would be challenged to provide answers to problems in the realm of public affairs. The nub of the matter appeared to

lie in achieving a more realistic awareness at large of the capabilities of psychology.

Associated with this process was the oftmentioned need for effective communication. The contention was made that the language of policy makers is not one that psychologists speak fluently enough; and, alternatively, it was said that the language of psychology may strike these others as so much jargon. Furthermore, the point was stressed that the assumptions regarding the "real world" may vary considerably as between those of contemporary psychology and the world of public affairs, and that psychology might represent a threat to entrenched assumptions.

These considerations, drawn from the Working Group's preceding activity, were a central feature of discussion when the standing Committee (Fred E. Fiedler, John L. Finan, Harold Guetzkow, E. P. Hollander, Charles E. Osgood, Joseph Weitz) convened for its first meeting in January 1961, some months after members had been appointed and organization had begun. To attain the long-range requirements of its mission, it was the consensus of the Committee that efforts be directed first toward the development of lines of communication, privately and quietly, with specific governmental agencies and congressional groups. These were to provide both for a better understanding of problems facing policy makers, in their own terms, and for means by which substantive materials and resources from psychology could be introduced as an additional feature in their decisions. Moreover, a commitment to scientific evidence and thought was considered to be paramount if the program was to achieve respect and avoid the appearance of special pleading.

Apart from the obvious need for personal contact, several supporting projects of varying character were programed in this first meeting. One of these was the task of bringing together in published form the research findings from psychology which have implications in particular for international tension reduction. Another was the collation of a roster of psychologists who regularly consult with government and therefore represent a reservoir of experience. Efforts were also considered to find means by which young psychologists might be brought to Washington, on perhaps a year's fellowship, to work in operating agencies and on congressional staffs where psychologists are not found. It was felt that this would have desirable instructional features in two directions. A plan was approved for holding conferences and seminars that would allow contact and an exchange of thinking between psychologists, governmental leaders, and figures from the mass media. Task forces, or subcommittees, were projected to work on a variety of public issues that might benefit from consideration of their distinctive

psychological features; a decisive aspect of this was a desire that relevant publics recognize that psychology has efficacy in matters of their concern. The common thread through all of these activities was the approach to a firmer and more congenial basis for psychology and the behavioral science orientation in the formulation of governmental policy.

The Committee was aware from the outset that any multipronged, sustained operation would require staff support through the APA Central Office, aside from the expenditure of effort by members of the Committee and others in the Association. Accordingly, explorations were almost immediately begun to find external funds to finance this operation on a full-time basis with assured continuity. Until that prospect was realized, a member of the Committee in the Washington area, E. P. Hollander, agreed to devote part of his time to service as the Committee's Executive Secretary, in establishing and directing a Committee office, sustaining and enlarging the external relationships required, and working with the APA's Executive Officer on the range of activities and functions programed.

Though it cannot claim every advance as a function of its efforts alone, the Committee has achieved an array of intermediate goals it set for itself. Communication has been established with the executive branch of government, as exemplified by the United States Information Agency, the Peace Corps, the President's Science Advisory Committee, and the new Arms Control and Disarmament Agency and its predecessor. A number of prominent members of the Association have had promising and repeated contact with figures in these and other segments of government. Useful and persisting relationships with various Congressional quarters also have been effected. In general, information of interest to the Committee is being gathered on a continuing basis in the Washington scene. And members of the Committee, more often as private citizens, have taken part in a host of conferences and other significant discussions which have provided still further operating information.

Through the Committee's Chairman, funds have been obtained from the Brookings Institution for several conferences involving behavioral scientists, governmental officials, and representatives from the mass media. A gift from Mr. Earl D. Osborn, President of the Institute for International Order, and a grant from the National Institute of Mental Health, have made possible the initiation of work on a comprehensive publication covering psychological research available and needed on international relations. Negotiations are presently being concluded for an initial two-year grant to fund a full-time staff member for the Committee in the APA Central Office.

Subcommittees are at work in several realms. One

has arranged a liaison with the office of the Secretary-General of the United Nations to find ways in which interested members of the Association may be of service. Another is supporting a working conference of behavioral scientists on the questions of a psychological and social nature that could be asked regarding the whole issue of a "shelter program." It is noteworthy that this last activity is being carried forth under the auspices of the Peace Research Institute, an organization in whose founding during the year the Committee played a part. For the upcoming year, a trial arrangement has been effected with the American Political Science Association to have one or more capable young psychologists take part in their continuing Congressional Fellowship Program, with a view toward our future involvements in this kind of enterprise.

For a number of reasons, then, we are encouraged as a Committee by our progress thus far. We continue to feel that our mission from the Association is best fulfilled by building confidence and understanding through private contacts, rather than by public pronouncements. When, at the 1961 meetings of the Association, the Council was confronted by requests to have the Association speak out on a particular issue of considerable public concern, our Committee deliberated this course with due attention to the earnest feelings represented. Our special report to the Board and Council carried the resulting policy recommendation, subsequently approved by them:

The Association should speak for the psychological profession on social and political issues only when psychologists have a professional expertise which is clearly relevant to the issues involved and when there is a substantial convergence of judgment among psychologists on the nature and im-

plications of relevant scientific data. There are, of course, many urgent issues in which the need for psychological knowledge is apparent. When such knowledge is not available, the Association should encourage research to foster its development. At all times, the Association maintains its traditional interest in having its members participate as individual psychologists and citizens in the presentation and discussion of psychological facts and ideas of psychology as they bear on current national and international problems.

As we begin to provide for further activities through the Committee, it is our intent and expectation that more and more APA members can be drawn into a variety of tasks. The urgency of the problems of our time makes this all the more necessary. We have already found a considerable degree of manifest interest among the membership, reflected in numerous ways, including the large attendance at symposia treating these issues, and the unsolicited requests to have a part in our projects. It is our desire to capitalize on these positive signs of concern, and we are now beginning a project to survey member interests and competences in the realm of our mission. Looking ahead, we see justification for the hope that persistent effort along these lines will yield continually increasing gains.

URIE BRONFENBRENNER

MORTON DEUTSCH

FRED E. FIEDLER

HAROLD GUETZKOW

E. P. HOLLANDER

JOSEPH WEITZ

CHARLES E. OSGOOD, *Chairman*

*APA Committee on Psychology in
National and International Affairs*

The APA Committee on Psychology in National and International Affairs is interested in identifying ongoing and proposed studies or other activities which have relevance to its mission. Individuals directing such enterprises are encouraged to send a one-page abstract of their project to the Chairman, Committee on Psychology in National and International Affairs, c/o APA, 1333 Sixteenth Street, N.W., Washington 6, D. C.

NEW JERSEY VIGNETTES:

FIFTY YEARS OF STATE PSYCHOLOGICAL SERVICES¹

HENRY P. DAVID

New Jersey State Department of Institutions and Agencies

NEW Jersey State Psychological Services began on October 1, 1910 when J. E. Wallace Wallin left the Vineland Training School to open a Psychological Laboratory at the New Jersey Village for Epileptics, located on a site near Princeton now occupied by the New Jersey Neuro-Psychiatric Institute. Midwife to the event was the Vineland Training School, a semiprivate institution founded in 1888 by S. Olin Garrison. Henry H. Goddard had established a Research Department in 1906 and with his Superintendent, Edward R. Johnstone, urged Wallin to accept the invitation to head the first tax supported psychological clinic in New Jersey.

Recalls Wallin (1955):

I did all of the individual and group testing myself, scored all the papers, made all the records, compiled all the results, did all the transcribing (as much as could be done) and did all the clerical work (filing, indexing, typing reports, letters, write-up of results and articles, and answering phone calls). I was not even given the use of a typewriter, but had to purchase my own machine on which I did all the official typing. I paid for my own stationery and stamps. I did practically everything except that I did not pilot patients to the laboratory or sweep the laboratory floor.

Wallin's initial laboratory equipment included the 1908 Goddard-Binet, the Vineland Seguin Form Board—both of which had been in use in the Vineland Summer School for Teachers—a universal dynamometer, the Smedley hand dynamometer, a spirometer, an ataxiograph, a pair of platform scales, and a stadiometer. Findings were subsequently reported in Wallin's early papers, compiled in his *Odyssey of a Psychologist*.

What were interprofessional relations in 1910? On his second day on the job Wallin was told that he was "not an M.D. and that my Ph.D. meant nothing in this institution." Adds Wallin: "None of the 4 physicians

had even a baccalaureate." Other volatile sessions with the medical superintendent are vividly recalled in Wallin's *Odyssey* (1955), recommended reading for anyone interested in the stormy history of psychologists' early efforts to attain appropriate recognition for professional services.

Wallin's struggle was a lonely one and he resigned in 1911. The New Jersey State Psychological Services remained dormant until February 28, 1919 when Edgar A. Doll conducted a mental survey of the entire population of 800 men at the New Jersey State Prison in Trenton, using the Army Alpha. He was fresh from the Psychological Examination Board at Camp Dix and still in his United States Army Sanitary Corps Lieutenant's uniform. During the next 4 months, Alpha was followed by Beta, personal histories, and individual examinations. The results of this project, including intellectual and vocational tests, statistical information, and social background data, were published in the 1919 Annual Report of the New Jersey State Prison. It became the forerunner of major New Jersey contributions to penology, such as the classification system for offenders subsequently adopted by the federal prison system and other state correctional centers.

Edgar A. Doll became New Jersey's first State Chief Psychologist, 1919-23. He later recalled how psychologists adapted their services to local needs, testing men where available, whether in road camps or in the death house. Examination of women offenders was considered "professionally hazardous" and female assistants were sought to obviate the need for a "body guard." Nevertheless, histories solicited by male examiners "were usually more reliable than those obtained by women."

Professional problems encountered in the twenties have an all too familiar ring. The volumes of the Vineland Training School *Bulletin* suggest that in 1920 Doll was concerned about the "improper use of the I.Q." and the desirability of a PhD degree in clinical psychology. In 1921, he wrote on the education of juvenile delinquents, suggested a classification system, published a monograph on the growth of intelligence, and commented on "Psychology and Psychiatry in Relation to School Hygiene." A year later he urged "study of the individual as a basis for institutional treatment," and proposed a "minimum psychological program for mental inspection in the public schools."

¹ The Fiftieth Anniversary of New Jersey State Psychological Services was celebrated on October 22, 1960 at a Princeton Inn Dinner co-sponsored by the New Jersey State Department of Institutions and Agencies and the New Jersey Psychological Association, honoring J. E. Wallace Wallin and Edgar A. Doll. Distributed on that occasion was a Commemorative Issue of the *New Jersey Welfare Reporter*, single copies of which are available on request to: Bureau of Publications, State Office Building, Trenton 25, New Jersey.

In 1923, his last year as State Chief Psychologist, Doll wrote about problems in mental diagnosis, classification of prisoners for training and parole, and the application of psychology to public health problems. Today, nearly 40 years later, these commentaries and research reports are still recommended reading.

The depression of the thirties curbed public expenditures in all areas, including psychology. Equipment, supplies, and money were hard to get; telephones, cars, and offices were pooled; and most everybody was expected to do double duty. New Jersey was fortunate in holding its gains under the strong leadership of James Quinter Holsopple, who served as Chief Psychologist from 1929-46. Perhaps typical of the times was Holsopple's 1936 memorandum announcing the first Civil Service Examination in several years for Assistant Psychologist. Starting salary was \$1,800 per year. Candidates were expected to have "an education equivalent to that represented by a Ph.D. in psychology, with a background in philosophy, education, and natural science"; should have had "graduate instruction in abnormal psychology, tests, measurements, and statistical method; and at least one year's experience in clinical work under competent psychological direction." There were limited promotional possibilities to Psychologist; starting salary was \$3,000.

The depression was followed by World War II and the booming postwar years. Holsopple served in the Navy, returned in 1945, and joined the Veterans Administration a year later. In New Jersey, as in many other states, 1932 salaries still prevailed; institutions worked on a 12-hour day, 72-hour week; and of the 13 budgeted state psychology positions, 7 were vacant. Lloyd Yepsen functioned as Chief Psychologist from 1946-50; he was followed by Albert Ellis until 1952 when Maurice G. Kott was appointed. Kott had joined the department in 1939 as an intern and eventually became the first Director of the Division of Mental Retardation, with the writer assuming the responsibilities of Chief Psychologist and Psychology Consultant in 1956.

New Jersey State Psychological Services, with their historic primary emphasis on testing, were strongly affected by the vast change in professional roles, influenced by military experiences, Veterans Administration training programs, and the growing public demand for active mental health facilities. Kott recalls that questions about the transition from intelligence testing to personality evaluations, psychotherapeutic programs, and administrative responsibilities persisted for a long time. However, by the end of the postwar decade clinical psychological services were expanding, with the full range of clinical training and research activities dependent on the local environment and the skills of the Director of Psychology concerned.

Another hurdle for the professional development of New Jersey psychologists was the traditional procedure of appointment and promotion by Civil Service process and examination. Insistence on advanced degrees was initially considered in conflict with the merit system and its implied promotional opportunities. It was not until late 1956 that requirements for admission to New Jersey Civil Service examinations for psychologists insisted on graduate degrees and that a doctorate in psychology was formally required for promotion to senior positions.

With the expansion of psychological services and slowly attained higher salary levels, the department continued to recognize its responsibility for training and inservice staff development. Internships in clinical psychological services and research had been available since the early twenties and remuneration gradually improved from the traditional room and board to annual tax-free stipends of over \$3,000. In recent years the program has been formalized with clearly established criteria for admission and supervision. In 1959 the APA Education and Training Board accredited the New Jersey Psychology Core Training Program, and in 1961 the National Institute of Mental Health awarded a 3-year grant to the department for the appointment of a Director of Psychological Training.

At this writing, the total number of budgeted and other state psychology positions in correctional centers, mental hospitals, and training schools exceeds 100, with nearly half the staff at the doctorate level. Also affiliated with the department are local community mental health centers, most of whose 50 psychologists hold the PhD degree. Full-time research positions, devoid of service responsibilities, have been created; staff and students from other lands are coming to New Jersey; and monthly *News Notes* keep everyone informed of state-wide activities. We welcome the newly established Clinical Psychology Training Program at Rutgers University and look forward to ever closer cooperation.

As we face the next 50 years, New Jersey state psychologists are aware that some of the problems encountered in 1910 are still with us. There is also an awareness that we are living in a time of rapid cultural change, re-evaluating ultimate responsibilities, and implementing recommendations of the Joint Commission Report on Mental Illness and Health. We are confident that psychological services will continue to widen in scope, responsibility, and tangible recognition. As it must have in 1910, the future beckons.

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Psychology in the News

Another "Monkey Trial"?

We Can Only Hope—

By mass communication standards those who follow such matters commonly acclaim Harry F. Harlow's presidential address and the research on which it was based as the most successful psychological sputnik of our time.

His report went to the front pages everywhere. It also went around the world, to wherever there was anyone who was interested in learning, or interested in mother, in sex, or in small, appealing animals, or in human behavior, or in psychology, in humor, or in doggerel.

It rather made one wonder who would not be interested?

Perhaps we can now answer that.

In any case, Senator Harry F. Byrd, Democrat of Virginia, in February of this year complained about the expenditure of federal money to study the "affectional relationship of an infant monkey and his mother." The economy-minded Senator also objected to the study of "biological control of snails by shell-cracker sunfish, and another for the study of diving reflexes and volume receptors in the seal."

We imagine Senator Byrd would be shocked to know that *many primates vote in Virginia*. And many of them, personally known to us, are going to take this whole thing as a slur on their deepest affectional responses. What better subject to contemplate than love of mother, huh? And why should not NIH be a mother to such a study?

Setting Us Straight on Methods In Mad. Ave. and House

Long, long ago, when JFK was first in the White House, this department quoted from a story in *Advertising Age* about market research on a particular coffee.

The product manager of a coffee company had told an audience that the company had picked two similar areas, and advertised twice as heavily in one of them. Then it was found that coffee sales were twice as good in the market where *less* money had been spent.

In the item quoted (*Amer. Psychologist*, February, 1961), was the manager's statement that

"researchers are unable to tell management which copy appeal is right, which medium to use or how much to spend on advertising."

This department made no comment on that. We asked for readers to do so, and they did.

Steuart Henderson Britt, Professor of Marketing and Advertising at Northwestern, raised the point of what acceptance could really be given to such a newspaper account of a luncheon speech.

John C. Maloney, manager of research development for the Leo Burnett Inc., advertising agency, wrote a letter which, as an ad man might say, was fairly long copy but punchy.

He thought the original was "a sticky experimental design problem" and that "a minimal requirement for such market tests demands that at least two markets be studied for each experimental treatment" (instead of the one market per treatment as described).

Maloney flatly denied the charge that research in this area does not help in judging advertising effectiveness.

He said:

Although functioning in a very imperfect manner in this area, advertising research has done a good deal to tell management "which copy appeal is right, which medium to use or how much to spend on advertising." Advertising research has also advised management rather regularly concerning: which advertisements are most apt to attract attention; which kinds of advertising will be best understood; which tactics to use for creating, changing or reinforcing attitudes about the product (we often call them "images" when we talk to our advertiser friends); which kinds of satisfactions people derive from buying, using or having used management's products. . . .

Mr. Maloney listed many other areas where research has been useful but said:

I do not mean to imply that all advertising researchers are capitalizing on all that psychology has to offer. I do hope to make it clear, however, that the deficiencies of the best advertising research merely reflect the blind spots in today's body of psychological knowledge.

The advertising researcher is in a business where this whole field of psychology is taken for real. He is in a position to tell psychology what its limitations are and to overcome a few of them too. Many of today's most knowledgeable authorities in the areas of perception and cognition of mass communications and social group interactions are among the ranks of advertising researchers.

A letter which seems to contain pro and con and perhaps the quality of mercy came to us from a wellknown hospital. The hospital's name is withheld to protect the guilty, and to avoid further confusion the coffee is called Brand Xi.

The APA member, a research man in the clinical area, made this comment:

I read that little shocker about [Brand Xi] coffee and, like all the rest of your readers, raised and pointed the accusing finger at these obtuse advertising men. Then, muttering some stunning and original phrases about the Madison Ave. mind and grey flannel thinking, I went up on one of our chronic wards (euphemistically referred to as Continued Treatment Service). There—

But let me tell you a brief tale about chronic patients and research on their treatment. As you no doubt know, it has been established beyond any doubt that Thorazine is more effective than barbiturates or placebo or reserpine on almost any chronic schizophrenic; also that Thorazine has no noticeable effect on chronic schizophrenics, especially if they are apathetic; and in addition, that Thorazine helps some of these patients and not others; and further, that Thorazine does no more than psychotherapy in a well-staffed hospital. The findings are clear and contradictory, overwhelming and ambiguous, convincing as well as confusing. But no matter what the findings in any particular institution, you will find beyond any doubt that the staff physicians go right ahead doing what they have always done: they give the stuff, note what happens, and then increase or decrease the dosage. Practically all psychiatrists are convinced that phenothiazine drugs are indispensable in a mental hospital—and they will hold to this view no matter what are the findings of the researchers down the hall.

Now what shall we say to the advertising men?

So much for the winds over our coffee break. For the time being—as for the past year or so—we shall say nothing more about it. We may also try in 1962 to watch out for the practice of saying things between the lines. As so many practitioners attest, some kinds of practice make imperfect.

* * *

Psychology on TV

The *Long Island Press* recently carried this amusing item:

Which child psychologist do you watch, Robert Young or Andy Griffith? On the saccharinated "Window on Main Street" last night, Young was busy butting into a high school couple's romance. He even went so far as to carry the ladder to the bedroom window off Main Street so the teenagers could elope. Why so indulgent? Young explained that the girl trusted him and he didn't want her to lose faith in the adult world. His advise (*sic*) was that fathers should listen to their daughters.

Later, on Griffith's show, Andy came out in favor of

cracking down on the kids when they are about to make mistakes. Said Andy, "You can't let a youngster decide for himself. . . . All a parent can do is say 'Wait, trust me' and try to keep temptation away."

Has anyone heard from Dr. Joyce Brothers on this?

Brothers, by the way, remains quite visible and audible in the New York area, and has undoubtedly become the psychologist whose photograph would be recognized by the most persons.

How to Get Quietly Stoned

Here's a way to get stoned for less than \$5.00, as described in a recent Washington advertisement:

"TOUCHSTONE"

What is a touchstone? It is a stone of secret calmness. Finely cut and exquisitely polished onyx. Pleasing to the touch, psychologists say they relax nerves and soothe daily tensions. Especially appealing to the doodler, key chain twirler, excessive smoker—just about everybody. \$4.95.

AP readers are no doubt thinking of slogans involving rocks in the head, or rocks in the pocket. The firm doing the advertising—a fashionable shop on a fashionable avenue—uses this one, A TRANQUILIZING AMULET.

Surely this is tops for the man who has everything—if he also will believe anything.

Admission: One Pin?

Psychologists in our hearing have reacted nay and yea to this item from the Newsletter of Psychologists in Private Practice:

A little *psi* in your lapel is an excellent conversation piece. Psi pins can be bought in 14K solid gold for \$5 each from the editor. Wear one. In due time people will know at a glance you are a psychologist.

The nay people feel most startled by the idea of psychologists using a pin to start conversation. For that purpose, said one, it would be sufficient to print a large vivid Rorschach blot on one's shirt or blouse.

Or perhaps on the back of the hand. And to you, say the yea people.

Cultural Trends Department:

Sex Recognition Division

The International Council of Psychologists, which formerly had "women" in its title and none but women in the organization, is still having some trouble making it known today that men are welcome to join. They made this change in 1959, from ICWP to ICP.

We are glad to help Josephine Ross take down this FOR WOMEN ONLY sign, the more so since the undersigned was recently roundly rebuffed at the annual meeting of the National Press Club. The latter organization, one FOR MEN ONLY, had discussed membership admission changes for an hour or so. Your correspondent then rose to ask, "Would it be in order to ask if the membership committee considered admitting women and do they have anything to report on this subject?" The president of the Press Club replied instantly, "They did not and the question is not in order." (Laughter).

That was that.

The President of the United States seems able, with the aid of friends, to jiggle the aplomb of the Cosmos Club and the Metropolitan Club, but rest assured the Press Club bar is in no danger of taking May Craig in with their Haig and Haig.

Only one member even said anything to this question after the meeting. His ominous prediction: "You are going to get the name of being a woman-lover."

Psions of the Times

The NIMH is circulating a small exhibit on suicide, "A Public Health Problem of Growing Concern." It features the work and photographs of Shneidman and Farberow.

A newspaper called *Newsday* on Long Island has a columnist who says a psychologist has advised that to stay youthful one should "stay limber and loving and a little bit crazy." The psychologist was not identified and very likely will not read this note—or write in. We can only assume that somewhere he exists, still limber and being loved.

Publishers Weekly carries this item:

"Science in Betting: The Players and the Horses" by Roy M. Dorcus of the University of California and E. R. DaSilva is a dark horse in Harper's best seller lineup. As players and bookies head south for winter racing, "Science in Betting" is in its fourth printing. Sales surged in the New York City area when Charles A. Wagner gave the book a fullpage review in the New York *Mirror* magazine

section just before the opening of Belmont Race Track. The book discusses scientific methods, tested for race tracks east and west, which can yield profitable betting systems.

After stories in the *New York Times* and in *Life*, Mortimer Feinberg realized he had something sensational in "Parents' Handbook," based on a survey in which he asked 550 freshmen to describe how parents helped or hindered their studies. Four out of five said parents hindered, and this and other items are being found very instructive as the book is mailed to parents of freshmen at City College.

Following our APA convention, the Air Force Association's annual meeting also highlighted psychology and human factors. In Philadelphia the Martin Company of Baltimore exhibited a "space capsule" showing six volunteers living under conditions simulating longterm space flight. AFA visitors could look through windows, and observe the "space crew." The project was directed by Dr. Milton A. Grodsky. The man at the next desk suggests that a good lobby display for our next convention would be to have psychologists under glass, where the public could see them.

The *Technical Recommendations for Psychological Tests and Diagnostic Techniques*, advanced by the APA, seem to have had widespread effects. Mr. Sven Rydberg, a Foreign Affiliate to the APA, informs us that the Swedish compulsory school system will adopt the recommended stanine scale for its marks.

Edwin Diamond, science editor of *Newsweek* and a frequent visitor to APA conventions, is author of a widely discussed book, *The Science of Dreams*. This is a rather thorough report on what psychology and psychiatry have found out—or conjectured—about dreaming. According to Diamond, he got started on this after reading a piece by Calvin S. Hall, one of those whom Diamond calls "dream collectors."

—MICHAEL AMRINE

Psychology in the States

In the Scheme of Things

Somewhere there must sit a member given to musing about who is the APA's Barry Goldwater, who its Paul Douglas or Wayne Morse, where, if psychology has one, is its UN, and how, in the Year of Our Association 1962 the scientific Right and professional Left can reach coexistence. While he fiddles, the APA mailroom sometimes burns as soothsayers among the members pass on their full quota of predictions. Some fear APA has crossed its Rubicon, others urge putting more feet in more doors lest they slam shut; all sound like honest men.

It is not unusual for the APA mailbag to carry in one day a letter from member *A* that unless post-doctoral training comes in, the profession may go out; from member *B* that if Master's degree psychologists are not used more effectively, we remain mighty unrealistic; from member *C* that each new APA Division opens yet another vista; and from member *D* that unless psychology stops fragmenting, the family tree may end up as woodchips in the forest of science. The problems of the three princes of Laos are not simple. Neither are those of APA, which sometimes become those of BPA.

The thornier the issue, the more likely it seems to get tossed into the lap of the Board of Professional Affairs. BPA tries to grasp the nettle firmly, sometimes gets stung, but enters its fifth year the wiser for it. In these times that try men's imaginations, the Board held its fall meeting in November.

The Larger World Outside. It is perhaps noteworthy that one BPA member had to miss the first session of the two-day meeting because of a prior commitment which involved neither the insurance problem nor a certification bill. Rollo May, who several weeks before had appeared on a national TV panel to discuss the interrelations of religion and the behavioral sciences, was this time addressing a meeting on fallout shelters, speaking of democracy, social values, freedom, and human dignity, according to *The New York Times*.

BPA found, too, that a member of its own Committee on Mental Retardation, William P. Hurder, had been named by President Kennedy to a 24-member panel planning new approaches to the problems of mental retardation. He seemed in good

company, with a half-dozen APA colleagues (Leonard S. Cottrell, Jr., Lloyd M. Dunn, Nicholas Hobbs, Darrel J. Mase, Anne M. Ritter, Harold W. Stevenson) serving as fellow-appointees.

Against this backdrop, the Board launched into problems closer at hand.

Wanted: One Gestalt or None. A recent critic of multiple-choice tests points out that an intelligent student sometimes picks the "wrong" answer because he sees more possibilities than the test-maker intended. Others can argue the point. But if BPA sometimes seems to make haste slowly, perhaps it might submit that the same phenomenon is at work. Be that as it may, during its fall meeting the Board customarily allows itself the luxury of a half-day's free-ranging philosophy. But perhaps the following snatches from its November report better tell the story.

The Board felt itself variously perceived. Psychologists in practice, lacking the anchorage provided by university affiliation, have voiced a real need to be heard organizationally within APA. . . . They have not been inarticulate (if the reversal of the BPA recommendation on insurance matters is an index) nor without channels of communication (with the growth of state associations and their direct representation in Council). The conscious attempt to get non-university members on the Board seems likewise a move in the direction of more balanced representation. Nevertheless, BPA reminded itself it still means many things to many men. One segment apparently sees the Board as essentially academic in composition, hence not fully understanding of its interests; another regrets what it considers an overemphasis on professional as compared with scientific concerns.

The solutions seem to lie less in continual reconsideration of the organizational structure of APA, more in looking to what kinds of problems are not being listened to.

At issue seems less the need to do specific things about specific problems and more the need to think ahead, to anticipate. Perhaps concern should be less with the content of decisions as such, more with the decision-making process itself.

These do not sound exactly like excerpts from a PTA meeting where the issue—whether 'tis nobler in the mind to buy curtains for the auditorium—is discussed in straightforward terms, the Ayes and the Nays stand up to be counted, and the curtains are bought or not. But psychologists seem to act like psychologists first and committeemen second, and

the habit of groping for the broader issue while coming to grips with the narrower persists.

Does Anything Get Done? Yes. BPA reviewed the lessons learned from the history of the American Board for Psychological Services and breathed life into the successor plan for state association directories (reported in November and December by *AP* and to state associations by memo). It discussed the new provisions for outpatient coverage of functional disorders under the APA Major Medical Plan and enlisted the help of the New York Society of Clinical Psychologists and the Los Angeles Society of Clinical Psychologists in defining "psychologist" for purposes of reimbursement. It heard a legislative review. It took note of the extensive surveys of the Oregon and Illinois state associations and studied the training program of the Los Angeles Society of Clinical Psychologists. It reviewed, reacted to, discussed the implications of yet other developments.

But always the whole looked bigger than the sum of its parts. What seemed like a simple actuarial matter in insurance turned out to involve such broader issues as directories of psychological services, questions of fees, internships, standards. The APA member raining lusty but well-intentioned blows on the back of APA for its failure to act promptly and surely in this or that, seemed less a man with an axe to grind, more a symbol of psychology's present-day involvement with things beyond mazes, medians, and mental health.

There is no presumption here that the Board of Professional Affairs tries to handle complaining APA members by giving them therapy. This could prove a circular proposition, when the latter themselves have been in analysis. But so have members of the Board. And when listened to with the third ear, some of the problems seem to have a fourth dimension. The presenting complaint does not necessarily turn out to be the real issue, and the fear of treating symptoms is always there. That is why, this time around, the Board of Professional Affairs felt justified in spending considerable time on the report of one of its committees dedicated to not only the new but the longer and harder look.

6 × 26. That is the kind of table the Committee on Scientific and Professional Responsibility had come up with in its effort to study in detail the social influences operating on the standards of psy-

chologists. First under the chairmanship of James G. Miller, now under Raymond A. Bauer, the committee has been hacking a conceptual model out of the face of psychology's own Mount Rushmore. What happens when 26 identifiable influences—peers, fund-granting agencies, scientific societies, codes of ethics, training institutions, employment standards, and 20 others—interact with 6 major functions—research, teaching, service to clients, public service, professional relations, and publication? As one committee member put it, the 156 cells come to represent the problems psychology needs to face up to during the next 20 years.

In a word, the committee suggests psychology may understand its contemporary problems better if it brings to them the scientific tradition of fitting smaller pieces into larger patterns. Thus, the report begins by attempting to define the meaning of responsible behavior in psychology, moves on to what it can learn from other professions. Here the committee mulled over scores of thoughtful letters solicited from distinguished professional people knowledgeable about the clergy, law, medicine, social work, education, engineering, and architecture, among others. It read the literature on the sociology of the professions. It attempted a "blow-up" of that cell of the table dealing with the influence of fund-granting agencies. It arranged for a subcommittee, under the chairmanship of Harold H. Kelley, to cull ideas and findings from the literature on primary group influences. And it came up with a 47-page report and 25 pages' worth of appendices which the Board of Professional Affairs felt could, properly edited and somewhat reorganized, be made available for eventual distribution.

The report is hardly academic.

Training criteria are avowedly intended to insure a supply of competent persons to serve the needs of the clients and employers. They often, however, have the incidental benefit to the members of the profession that the training hurdle actually cuts down the supply of professionals and insures employment for those already admitted. Some of our correspondents cynically pointed out that in other fields training and admission standards were tightened during the Great Depression when employment was scarce.

Nor is it stuffy.

The persisting image of the professional is that of a knight on horseback who sallies along into combat bearing his client's colors. Specifically, he is seen as an individual artist, acting creatively, in intimate contact with his client, and receiving as a reward his fee. In practice he may be a member of a large organization, doing a circumscribed al-

though demanding job, having no contact with the client, and receiving a salary. In many instances the image envisioned by the general public, taught in school, and held by the client and professional himself is not congruent with the reality of practice. This disparity between the romanticized role of the professional and the reality of present-day practice produces confusion, frustration, guilt, and consequent misdirected effort. This is an instance of cultural lag, the responsibility for which lies in many quarters. Most pertinently, the schools in which professionals are trained generally perpetuate the romantic false image.

Least of all does it pull punches.

There is no reason to doubt that the majority of members of any profession wish to help their fellowmen. At least this is always the professional image. One does not need to be a full-blown iconoclast, however, to observe that not all the controls set up by professions primarily benefit the client. Nearly all steps toward professionalization are justified as protecting the client's, or the employer's interests. The initiative, however, has almost invariably come from inside the profession, and the action usually protects the interests of the members of the profession as well.

The committee has sought neither to pontificate, to legislate, nor to overstate. It has tried to break ground on some major issues so others might better dig in. There is no claim to originality; other professions have thought about and written on the problems. But neither is there the feeling of being Peck's Bad Boy; the same other professions need to make peace with themselves about the same issues.

* * *

Cell #29. Somewhere in the master table of the Committee on Scientific and Professional Responsibility there rests at least one cell intended to accommodate the spirit which fires the Los Angeles Society of Clinical Psychologists. For two years, its Educational Planning Committee has held fortnightly meetings, hatching its program for a high-level, long-range, postdoctoral training program in clinical psychology. The period of incubation over, LASCP is now preening the still wobbly, wet-feathered fledgling with the care one-time parents lavish on their young. The pedigree reads:

The EPA conceives the aim of a postdoctoral training program to be the development of professional clinical psychologists with a high degree of professional competence, dedicated to advancing scientific knowledge, and scrupulously responsive to the best interests of the community.

Such a program ought to nurture that kind of clinician who is able to assume the responsibility of independent action, competent to translate abstract knowledge into meaningful practice, effective and discriminating in the use of whatever psychological tools, techniques, skills and knowledge are effective in the diagnosis and the resolution of human personality problems.

To the Board of Professional Affairs, the carefully drawn strategy of the LASCP committee under the chairmanship of Hedra Bolgar seemed commendable. Immediate considerations were not being allowed to obscure the bigger picture nor courses on techniques left to choke the curriculum. And this, as BPA wished the budding venture well, seemed to be practicing what is sometimes only preached.

* * *

Neither Hidden nor Persuaders. Each year the Psychology Examining Committee of the California State Board of Medical Examiners is required to give an account of its stewardship. The latest report was submitted to the Honorable Edmund G. Brown, Governor of California, at the time 2,467 psychologists had been certified in the State. Of all these, only one had required disciplinary action involving revocation of the certificate, and there, in fact, the offense was apart from the individual's record as a psychologist.

With some little pride, the report to the Governor concludes:

It appears, therefore, that as a group psychologists are adhering to the high standards required by their professional code of ethics and the laws of this State. In a profession which has moved only recently from preoccupation with teaching and research into professional practice as well, this is an especially good record. It supports the wisdom of the State's lawmakers when, in 1957, they recognized the imperative need to enact legislation that would enable the public to be reasonably well assured that those who call themselves certified psychologists are persons with adequate academic training and personal qualifications.

Something, we would say, maybe even Mr. Packard will find reassuring.

—ELIOT H. RODNICK

Chairman
Board of Professional Affairs
ERASMUS L. HOCH
Administrative Officer
State and Professional Affairs

Notes and News

In the Call for Papers (*Amer. Psychologist*, 1961, 16, 737) an incorrect address was listed for William U. Snyder, Program Chairman for Division 12. The correct address is: Department of Psychology, Burrowes Building, The Pennsylvania State University, University Park, Pennsylvania.

The Eastern Psychological Association will operate a Placement Office at its annual meeting in Atlantic City, New Jersey, April 26-28, 1962. The Placement Office will be located in the Vernon Room on the Lounge Floor of Haddon Hall. General inquiries about the placement facilities should be addressed to the Chairman of the EPA Placement Committee: Dr. Douglas G. Schultz, Applied Psychological Services, 114 N. Wayne Avenue, Wayne, Pennsylvania. Employers and applicants are urged to preregister for this placement service, and forms may be obtained by writing to: Dr. Robert F. Lockman, Manpower Resources Division, APA Central Office, 1333 Sixteenth Street, N.W., Washington 6, D. C.

The Midwestern Psychological Association will operate a Placement Service Office at its annual meeting in Chicago, Illinois, May 3-5, 1962. The Placement Office will be located on the third floor of the Morrison Hotel adjacent to the exhibit area. General inquiries about the placement facilities should be addressed to the MPA Placement Chairman: Dr. Henry J. Lambin, Department of Psychology, Loyola University, 6525 Sheridan Road, Chicago 26, Illinois. Employers and applicants are urged to preregister for this placement service, and forms may be obtained by writing to: Dr. Robert F. Lockman, Manpower Resources Division, APA Central Office, 1333 Sixteenth Street, N.W., Washington 6, D. C.

The following regional psychological associations also will operate placement offices at their spring 1962 meetings.

Southeastern Psychological Association

March 29-31, 1962; Hotel Sheraton, Louisville, Kentucky; Placement Chairman:
Dr. Norman Willard

Director of Research
US Army Armor Human Research Unit
Fort Knox, Kentucky

Southwestern Psychological Association

April 5-7, 1962; Hotel Texas, Fort Worth, Texas; Placement Chairman:
Mr. Kenneth C. Hageman
2044 Glenco Terrace
Fort Worth, Texas

Western Psychological Association

April 19-21, 1962; Sheraton-Palace Hotel, San Francisco, California; Placement Chairman:
Dr. John Del Torto
Division of Psychology
San Francisco State College
San Francisco 27, California

Southern Society for Philosophy and Psychology

April 19-21, 1962; Memphis, Tennessee; Placement Chairman:
Dr. Edward A. Bilodeau
Department of Psychology
Tulane University
New Orleans, Louisiana

Rocky Mountain Psychological Association

May 10-12, 1962; Butte, Montana; Placement Chairman:
Dr. Frank M. du Mas
Department of Psychology
Montana State University
Missoula, Montana

Applicant and general inquiries about placement facilities *should be addressed to placement chairmen*. Employers are urged to preregister by writing for forms to: Dr. Robert F. Lockman, Manpower Resources Division, APA Central Office, 1333 Sixteenth Street, N.W., Washington 6, D. C.

The American Board of Examiners in Professional Psychology, Inc., publicly expresses its appreciation to the following Diplomates who served as Readers of essay questions of candidates taking its written examinations in November, 1961:

Dorothy V. Anderson
 Gordon V. Anderson
 Lawrence M. Baker
 Bernard M. Bass
 Virginia Lee Block
 Douglas D. Blocksmas
 Roy Brener
 Rex M. Collier
 Orlo Crissey
 Henry P. David
 Joseph G. Dawson
 Austin DesLauriers
 Cynthia Deutsch
 Marvin D. Dunnette
 Jerome H. Ely
 Royal B. Embree
 Thomas N. Ewing
 Erika Fromm
 Clayton d'A. Gerken
 F. Harold Giedt
 Leo Goldman
 Leonard D. Goodstein
 Leon Gorlow
 Donald L. Grant
 Zoltan Gross
 John M. Hadley
 Jesse G. Harris, Jr.
 Norman I. Harway
 Edwin R. Henry
 Leota Long Janke
 Walter F. Johnson
 Marvin W. Kahn
 Lawrence Katz
 Raymond A. Katzell
 Barbara A. Kirk
 Irwin J. Knopf
 William S. Kogan
 Carl G. Lauterbach
 Wilbur L. Layton

Leslie F. Malpass
 Stanley I. Marzolf
 Joseph D. Matarazzo
 Laurence S. McGaughan
 William McGehee
 Herbert H. Meyer
 Henry H. Morgan
 Edwin C. Nevis
 Robert S. Nichols
 Jerry Osterweil
 Horace A. Page
 Robert Perloff
 Irene R. Pierce
 Gordon E. Rader
 Edward T. Raney
 Marvin Reznikoff
 Bernard F. Riess
 Jay T. Rusmore
 Joseph Samler
 Robert B. Selover
 E. Joseph Shoben, Jr.
 Irving Simos
 Kinsley R. Smith
 John A. Stern
 Joseph Stubbins
 Norman Sundberg
 Kenneth S. Teel
 Clare W. Thompson
 Joseph Tiffin
 Leona E. Tyler
 David N. Ulrich
 Edith Weisskopf-Joelson
 Charles Wenar
 A. G. Wesman
 Harold Wilensky
 Robert Wirt
 Irla Lee Zimmerman
 Marvin Zuckerman

The deaths of the following members have been reported:

Charles R. Atwell, November 1, 1961.
 Charles A. Coburn, June 18, 1961.
 Hedda Gorog, November 30, 1961.
 Jacob D. Heilan, October 21, 1960.
 Harold E. Israel, October 7, 1961.
 Hedwig Jahoda, December 22, 1961.
 Ursula M. Knoll, November 24, 1961.
 David I. Macht, 1961.
 Donald C. Moser, July 25, 1961.
 Martin Scheerer, October 19, 1961.
 Robert C. Schon, November 4, 1961.
 Sidney Siegel, December, 1961.
 Charles L. Stone, December 4, 1961.
 Raymond H. Wheeler, August, 1961.

On the basis of faulty information, the May 1961 issue of the *American Psychologist* erroneously reported that Brett R. Stuart had died on November 4, 1960.

Aerojet-General Corporation announces the appointment of Robert B. Morton, former Chief of Psychology Services at the VA Hospital in Houston, Texas, as Manager of the Management Development Program of their Liquid Rocket Plan in Sacramento, California.

The American Academy of Arts and Sciences, Boston, has awarded the Academy Monograph Prize in physical and biological sciences to George A. Talland for his study entitled, "The Amnesic Syndrome: A Psychological Study."

Donald W. MacKinnon, Professor of Psychology at the University of California, Berkeley, and Director of the Institute of Personality Assessment and Research, has been selected by the APA as the Walter Van Dyke Bingham Lecturer in 1962. The lectureship, established by Mrs. Bingham in honor of her husband, has the purpose of recognizing each year a psychologist and an institution which have made a significant contribution to the study of talent. Yale University is the institution honored this year, and the Lecture will be given there on April 11.

MacKinnon has also recently been appointed a member of the Mental Health Training Committee of the National Institute of Mental Health to serve as a member of the Subcommittee on Psychology.

Marian R. Ballin, formerly with the VA Regional Office in San Francisco, has been appointed School Psychologist for Placer County, Auburn, California.

Russell C. Bowers, formerly Counseling Psychologist for the Veterans Administration, has accepted a position on the counseling staff at the University of Notre Dame.

The California State Department of Mental Hygiene has announced the following appointments to its Research Division:

Ernest H. Dondis, Research Psychologist in charge of the Treatment Program Evaluation Section
 Robert T. Ross, Acting Chief of Research.

Catholic University of America has announced the appointment of **Clement J. Grasock** as Director of the University Counseling Center.

City College of the City University of New York announces the appointment of **George A. Kelly**, of Ohio State University, as the Buell G. Gallagher Visiting Professor during the spring of 1962.

Edward Clifford, formerly at the Denver Child Research Council, is now at the Children's Asthma Research Institute and Hospital, Denver, Colorado.

Cornell College, Mount Vernon, Iowa, has announced the appointment of **John R. Tisdale** as Assistant Professor of Psychology.

Alfred R. Fregly has joined the staff of the Naval School of Aviation Medicine, Pensacola, as Research Psychologist.

Harless and Kirkpatrick Associates, of Tampa and Atlanta, announces the appointment of **Melvin P. Reid**, formerly with the Florida State Board of Health, as staff psychologist in the Industrial Division.

The Iowa State Mental Health Institute announces the following appointments:

James W. Layman, Director, Department of Psychology
George F. Castore and **Kenneth McCullough**, full-time staff members

Alfred Heilbrun of the Department of Psychology, University of Iowa, as visiting consultant.

Leonard L. Mitnick has joined the staff of the Information Systems Operation, General Electric Company, Bethesda, Maryland.

Samuel Hirschenfang has been appointed Clinical Psychologist in the Department of Physical Medicine and Rehabilitation at Kings County Hospital Center, Brooklyn, and Clinical Assistant Instructor in the Department of Rehabilitation Medicine, State University of New York, Downstate Medical Center, Brooklyn, New York.

The Psychology Department of Long Island University in Brooklyn, New York, announces the following appointments for the current academic year:

Edward I. Strongin, Professor
Ruth Hartley, Associate Professor
Jerome Nagel, Assistant Professor

Albert P. Maslow, Chief of the Test Development and Occupational Section, Standards Division, Bureau of Programs and Standards, U. S. Civil Service Commission, was presented last September with the First Annual Distinguished Science Award by Psychologists in Public Service, Division 18.

Robert C. Nichols, formerly Assistant Professor of Psychology at Purdue University, has been appointed Research Associate in the Research Division of the National Merit Scholarship Corporation.

Robert B. Payne has been appointed Director of Medical Research at the School of Aerospace Medicine, Brooks Air Force Base, Texas.

Richard G. Pearson, formerly with Aviation Crash Injury Research, Phoenix, Arizona, has joined the Psychology Branch, FAA Civil Aeromedical Research Institute, as Chief, Psychophysiology Section.

Public Service Research, Inc., Stamford, Connecticut, announces that **Robert J. Schreiber** has been elected President and Executive Director of Research.

Martin Reiser has been appointed Psychologist, Division of Counseling, The Pennsylvania State University, Ogontz Campus, Abington, Pennsylvania.

Rohrer, Hibler, and Replogle has announced the appointment of **Charles N. Newstrom** to the Staff of their Toronto office.

Lee Sechrist, of Northwestern University, has been awarded a grant by the Public Health Service for an experiment with children at the Glenkirk School for Retarded Children.

George Soloyanis, formerly with the Bureau of Mental Health, D. C. Department of Public Health, has accepted a position as Program Specialist, the Ford Foundation.

Ross Stagner has accepted a National Science Foundation Fellowship for a six months' study of international economics. While he is on leave, **Donald N. Elliott** will be Acting Chairman of the Department of Psychology at Wayne State University.

Gerald L. Strizver has joined the staff as Clinical Psychologist at the Pittsburgh Child Guidance Center.

The System Development Corporation of Santa Monica, California, announces the following appointments:

Stewart E. Fliege, Manager of the SAGE Computer Programming Development Department

Robert Harrington, Manager of the Bio-Medical Systems Department.

Irving A. Taylor, formerly Director of Research at Nowland and Company, has accepted a position as Associate Professor of Psychology at San Fernando Valley State College, California.

The Stanford University School of Medicine announces the appointment of **Joseph R. Handlon**, formerly Research Psychologist at the National Institute of Mental Health, as Associate Professor of Psychology in the Department of Psychiatry.

Stanford University announces the appointment of **William R. Estes**, now at Indiana University, to begin September 1962.

The University of Southern California, Los Angeles, announces the appointment of **Mark S. Mayzner**, formerly of Bell Telephone Laboratories and Barnard College, as Assistant Professor of Psychology.

The University of Vermont announces the appointment of **Mervyn W. Perrine**, formerly Dozent and Director of the Research Center for Visual Perception at the Hochschule für Gestaltung in Ulm, Germany, as Assistant Professor in the Department of Psychology.

The University of Virginia School of Medicine announces the appointment of **Joel Greenspoon**, Professor of Psychology at Florida State University, as Visiting Professor of Behavioral Science with the Behavior Research Laboratories of the Division of Behavioral Science for the Spring, 1962 semester.

Yale University's Department of Psychology announces the appointment of **Robert Galambos**, formerly Chief of Neurophysiology at the Walter Reed Army Medical Center, as Eugene Higgins Professor of Psychology. He will hold a similar title in the Department of Physiology in the Medical School.

The Department of Experimental and Clinical Psychology, Graduate School of Education, Yeshiva University announces the following appointments:

Irvin Rock, Professor in Psychology

Sybil S. Barten, Assistant Professor in Psychology

Solomon E. Asch, Visiting Professor in Psychology

Goldie R. Kaback, Visiting Associate Professor in Psychology

Leonard S. Kogan, Adjunct Professor in Psychology

Ruth Lesser, Lecturer in Psychology.

The following rosters of officers have been announced:

Central Iowa Psychological Association

President: Leo Subotnik

President-elect: Carl Morgan

Past President: Marilee Fredericks

Secretary-Treasurer: Art MacKinney

Los Angeles County Psychological Association

President: Nicholas Rose

President-elect: Launor F. Carter

Past President: William W. Grings

Secretary: James M. Holt

Treasurer: Albert E. Ross

Texas Psychological Association

President: Charles C. Cleland

President-elect: Carson McGuire

Past President: Ruth Hubbard

Secretary-Treasurer: H. Paul Kelley

Parliamentarian: Ray Fletcher

The following list of pro tem officers has been announced:

Psychologists Interested in the Advancement of Psychotherapy:

President: Eugene T. Gendlin

Past President: Arthur H. Davison

Secretary: Leonard Pearson

Treasurer: Anita C. Montague

APA's ad hoc **Committee on Mental Retardation** announces the availability of stipends for university faculty members who wish to carry on postdoctoral study in institutions for the mentally retarded or similar institutions. The senior stipend program (Training Branch) and the fellowship program (Research Grants and Fellowships Branch) are both available from the National Institute of Mental Health, Bethesda 14, Maryland.

Postdoctoral fellowships are for instructors or assistant professors of less than three years postdoctoral experience; special fellowships, for the more experienced, are awarded only for research

training. Senior stipends are awarded to specialists in a mental health discipline who wish to broaden their training for other purposes.

St. John's College, Cambridge, England, proposes not later than the end of April 1962 to make an election to the Kenneth Craik Research Award for assistance to persons engaged in postgraduate research, preferably in physiological psychology. Persons of either sex and of any academic standing are eligible. The award recipient need not be and need not become a member of the College and need not reside in the University, but shall submit such reports on the place, nature, and progress of his research as the College Council may require. The award shall be tenable, subject to the consent of the Council, with any other emolument and with a salaried post.

The value of the award will be £500 a year. The Council may pay to the recipient such part of the award as they shall decide in the form of a grant or grants to meet specified costs incurred by the research undertaken; to include travel costs, purchase or hire of apparatus or equipment, or of provision of technical or clerical assistance. The award will be tenable from 1 October 1962 for such period as the Council shall decide, of not less than one year and not more than three; if the election is for less than three years, tenure, at the discretion of the Council, may be prolonged for further period or periods, provided the award shall not be held by the same person for more than three years in all.

Applications should be sent to The Master, St. John's College, Cambridge, England, to reach him not later than 1 April 1962, accompanied by full particulars of the applicant, statement of the nature and probable duration of the contemplated research and where it is to be carried out, particulars of further financial assistance anticipated by the applicant, and the names and addresses of not more than three references. Do not send testimonials.

Two fellowships in *Parapsychology* are being offered in the Department of Psychology, **City College of New York**, for graduate work leading to the Master's degree in psychology. Each fellowship provides \$1,500 and free tuition. Recipients must qualify for matriculation in the graduate program of the Department of Psychology, must meet all course requirements for the MA, and must conduct research in parapsychology for their masters' theses. For information and application forms write to: Dr.

Gertrude Schmeidler, Department of Psychology, City College of New York. The closing date for applications is May 15, 1962.

Psychologists below the age of 40 with PhD and three years postdoctoral experience who wish to specialize in the field of *community mental health* may apply for training at the **Harvard School of Public Health**. Two or three new students who fulfill the general admissions requirements may be accepted each year. Basic course of study is 1-3 years, and may lead to the degrees of Master or Doctor of Science in Hygiene (Community Mental Health). The emphasis in this Program is on the integration of public health and mental health concepts. Students' time is divided between course work and seminars in public health and specialized work in mental health. In the latter category particular emphasis is placed on mental health consultation, mental health education, community organization, community research, and other aspects of community psychiatry. Fellowships are available from the National Institute of Mental Health and from the Grant Foundation of New York. For the 1962-63 Class candidates are urged to submit applications before April 1, 1962; however, applications completed by July 31, 1962 will be processed subject to availability of space. All inquiries in regard to the Program should be addressed to Dr. Gerald Caplan, Community Mental Health Program, Harvard School of Public Health, 55 Shattuck Street, Boston 15, Massachusetts.

The **Paul R. Mort Fellowship in Educational Administration**, a \$3,840 one-year fellowship, has been announced by Teachers College, Columbia University for the academic year 1962-63. It will be awarded to an outstanding young superintendent of schools, supervising principal, or prospective chief administrator for a year's full-time study leading to a doctor's degree in the Department of Educational Administration at Teachers College.

The Institute of Administrative Research, which will administer the fellowship, has also announced the availability of 12 research assistantships for a year's study in educational administration, each worth \$2,400. Applicants for these grants must intend to make educational administration or administrative research their life's work.

Applications for the fellowship and assistantships are obtainable from the Institute of Administrative Research, Teachers College, Columbia University,

New York 27, New York. The recipients will be named in April.

Michigan State University this year again has available several three-year *NDEA Fellowships in Research Design and Development*. Educational psychology students may work toward both the masters and doctoral degrees. The stipend is \$2,000, \$2,200, and \$2,400 for the first, second, and third years respectively, plus \$400 a year for each dependent. Applicants should have completed no more than one half-year of graduate study, and have a background in education or one of the other behavioral sciences, or in mathematics or statistics. Inquiries should be addressed to: Educational Research Design and Development Fellowship, Bureau of Educational Research, Michigan State University, East Lansing, Michigan.

The Pennsylvania State University has been awarded a National Science Foundation Grant in support of a summer program of "Research Participation for College Teachers." Further information can be obtained from William M. Lepley, Department of Psychology, 112 Burrows Building, The Pennsylvania State University, University Park, Pennsylvania.

The National Science Foundation announces changes in the *Graduate-Level Research Facilities Program*. Educational institutions having graduate programs in the life and social sciences, among others, and offering at least the master's degree in these areas, are now eligible. Funds may now be requested for general purpose research equipment up to a maximum of 10% of total funds otherwise requested in the facility proposal. Nonprofit, non-degree granting research institutions are now eligible provided they are associated with institutions of higher education in matters of graduate research and training. Copies of the brochure outlining requirements for submission of proposals are available without cost upon request to the Office of Institutional Programs, National Science Foundation, Washington 25, D. C.

Young scientists who wish to obtain training and experience in *research methodology in the field of mental health*, are invited to apply for United States Public Health Service Clinical Research Fellowships under **Menninger Foundation** sponsorship. Qualified research Fellows would be assigned to the

Menninger Foundation Research Department, which includes a staff of eight psychologists, three psychiatrists, ten research assistants, two statisticians, two biomedical engineers, and a large group of psychiatrists, psychologists, and social workers from other departments who share in formal research projects under Research Department auspices. Research problems under investigation range from factors responsible for the success or failure of various forms of psychotherapy, to experiments on idiosyncratic cognitive styles, the influence of incidental stimuli on images and dreams, the ways in which normal children cope with their problems, and studies in perceptual biases and reality testing. The Research Fellow may work within the framework provided by one of these projects, or he may pursue supervised research on interests of his own, provided the area he selects is consistent with the general research program of the Foundation. The combined USPHS and Menninger Foundation stipend totals \$6,000 for the first year and \$7,000 the second year, tax free. For further information or application forms write to Dr. Gardner Murphy, Director of Research, The Menninger Foundation, Topeka, Kansas.

The University of Utah has received a grant from the National Science Foundation to organize and extend the *Utah creativity conference series*. The first phase, which is nearing completion and being read by senior reviewers, is to produce a 1961 report summarizing the research state of knowledge and indicating some of the more urgent needs and promising leads for further research. The subtopics covered are: predictors of creativity, creativity in education, creativity training programs and climate variables, and criteria of creativity. The second phase is to hold another research conference on creativity in 1962 (similar to the 1955, 1957, and 1959 Utah conferences) designed to extend the research knowledge beyond that summarized in the 1961 report. As before, the participants will be selected by a steering committee. Please let us know of any creativity research completed or in progress by psychologists or others; write to: Calvin W. Taylor, Principal Investigator; Department of Psychology, University of Utah; Salt Lake City, Utah.

The Menninger Foundation is now carrying on nine research projects, utilizing seven full-time psychologists and the part-time services of a large num-

ber of psychologists, psychiatrists, social workers, and biomedical engineers, in experimental, clinical, and developmental projects:

The Psychotherapy Research Project, a long-range study of the interaction of patients and therapists, in terms of both the outcome and the dynamics of the treatment, with follow-up two years after the termination of treatment; the Thyroid Project deals with the relation of "hot nodules" of thyroid tissue to personality patterns and dynamics; the Cognition Project (formerly the Perception Project) deals with broad and persistent individual differences in perceptual-cognitive style; the project in Personality Organization compares clinical personality assessments with experimental determination of perceptual styles; the project on Fleeting Perception in Dreams and Images uses a series of new techniques, including a rebus technique and a hypnotic technique, for the identification of visual materials which are tachistoscopically exposed but not reported under conditions of verbal recall; the Reality-Testing Project (formerly the Perceptual Learning Project) deals with the ways in which veridical and nonveridical perceptual responses arise and the ways in which they can be unlearned or extinguished, and how the resulting reality-testing skills can be generalized; the Coping Project comprises longitudinal studies of children from infancy to their present 12-year-old level in terms of the ways in which children meet severe difficulties and challenges; the Urban Renewal Project deals with the influence of counseling service upon the well-being of persons displaced from their homes in the course of Urban Renewal, in comparison with control groups which receive no such counseling service; a project has just been established on Creativeness in Relation to Extrasensory Perception which involves support of and collaboration between five investigators working in various American research centers, to determine whether certain aspects of creative thinking are relevant to certain extrasensory phenomena, obtained in biographical and likewise in experimental situations.

The American Association of Instructors of the Blind has established a Research Advisory Committee to propose, stimulate, and review proposals; provide consultation on research or experimental design and for applicants for financial grants for the *conduct of educational or psychology* research with the blind. These services will be available to all agencies and school programs as well as to individuals from those programs, and to all faculty and students of graduate programs in education or psychology who are considering or planning educational or psychological research projects with or for the blind. Any individual or organization seeking the services of the committee should write to Carl J. Davis, Chairman, Research Advisory Committee, Perkins School for the Blind, Watertown 72, Massachusetts.

Announcement has been made of the foundation of the **Colombian Association for the Study of Clinical Psychology**. Among its various purposes is that of serving as an information source for foreign colleagues on the development of psychology, particularly clinical psychology, in Colombia. Guido Wilde is the first president.

The **Galton Institute** has been established for the purpose of conducting research into the nature and development of creativity in children. It is a nonprofit, tax-exempt organization, legally incorporated under California law. Inquiries are invited both from persons interested in becoming members of the Institute and from qualified researchers interested in the scientific exploration of cognition and creativity in children. For further information, please communicate with Frieda B. Libaw, PhD, Executive Director, The Galton Institute, 10400 Wilshire Boulevard, Los Angeles 24, California.

Formation of the **Human Interaction Research Institute** as a new nonprofit corporation, with headquarters at 595 East Colorado Boulevard in Pasadena, has been announced.

The Institute has been formed for educational and scientific purposes, and for the specific and primary purpose of organizing, conducting, sponsoring, and directing research and education in human behavior, for the public benefit. Particular emphasis will be placed upon (a) the identification, development and utilization of talent; (b) the study of man-man, man-group, and man-machine relationships; (c) organizational arrangements that facilitate effective task performance; (d) the identification, exposure and more effective transmission of relative excellence in ways of coping with major common problems in American life; and (e) the stimulation of scientific discussion and the development of theory in those fields. The Board of Directors consists of four psychologists and three non-psychologists.

The **Survey Research Center** of The University of Michigan will hold a Summer Institute in Survey Research Techniques from July 23 to August 18, 1962, for the regular session, and with introductory courses from June 25 to July 21. For further information, write to the Survey Research Center, The University of Michigan, Ann Arbor, Michigan.

The University of Mississippi Medical Center, Jackson, Mississippi, announces a postgraduate lecture series, "Social Science in Medicine," to be held during 1962-63 at the University Medical Center on the following Fridays at 1 p.m.: January 12, Nonspecific Forces Surrounding Disease and Treatment of Disease; March 16, Psychology of Suicidal Behavior; May 4, The Subculture of Medicine; October 12, Mental Health and Religion; January 1963 (to be announced). Psychologists are invited to attend.

The Departments of Psychology and Sociology-Anthropology of the University of Missouri are jointly sponsoring a conference on *The Propositional Structure of Role Theory* under contract with the Office of Naval Research. Two days of public meetings will be held in Columbia on March 1 and 2, 1962 with papers by major contributors to the role field both read and discussed. Copies of the papers to be presented became available about February 10, 1962. Requests for copies or additional information should be addressed to: Bruce J. Bidle, Associate Professor of Psychology and Sociology, Social Psychology Laboratory.

The Training Institute of the National Psychological Association for Psychoanalysis announces that applications for enrollment are now being accepted for the coming year. The NPAP Institute offers a comprehensive four-year program of psychoanalytic training which is designed to prepare qualified applicants for the professional practice of psychoanalysis and for admission to membership in the NPAP. Tuition loans and scholarship assistance are available. For further information write to the NPAP Registrar, 26 West Ninth Street, New York 11, New York.

The National Reading Research Foundation (NRRF) has announced a call for research proposals on the *psychology and sociology of reading*. The NRRF is limited to the support of basic (as opposed to applied) research and is particularly interested in entertaining projects designed to explore some of the psycho-physiological substrates of the reading process. Suggested areas of study include cerebral hemispheric dominance and its relation to reading ability; silent reading and silent (or inner) speech; endocrine function and its effects on reading ability; common factors of super-normal (high speed) readers; visual cortical suppression

and reading ability; association time, closure, and reading ability; ocular motor apraxia and reading difficulties. There is no closing date nor is a special form required for the submission of proposals. A complete statement describing the nature and purpose of the research, with a budgetary statement amended, is sufficient. All proposals should be addressed to the Chairman, Research Advisory Committee, National Reading Research Foundation, Tudor Arms Hotel, Cleveland 6, Ohio.

The Department of Psychiatry of the New York Medical College-Metropolitan Hospital Center, announces the establishment of an *internship program in Clinical Psychology* at Metropolitan Hospital, New York City. The program will offer an intensive clinical experience in a general hospital with active in-patient and out-patient psychiatric facilities. Interns will rotate through a number of services, among which are an Adolescent Out-Patient Clinic, Psychosomatic Clinic, Child Guidance Clinic, Drug Addiction Service, general psychiatric ward facilities, etc. The internship program includes didactic courses and intensive supervision. Applications are being accepted for appointments beginning about September 15, 1962. Address inquiries to: Dr. Morton Bard, Chief Psychologist, Department of Psychiatry, Metropolitan Hospital, 1901 First Avenue, New York 29, New York.

An organization called **Social Scientists for Peace**, its membership consisting of behavioral scientists, has been formed in the New York area. It will have as a division, *Psychologists for Peace*. The purpose of the group is to use the specific skills of its members to create a widespread climate of expectancy of peace. Local, national, and international organizations may reach the New York group by writing to: Stanley Zuckerman, 104-40 Queens Boulevard, Forest Hills 75, New York.

The Human Factors Research Branch (formerly the Personnel Research Branch) has been transferred to the office, Chief of Research and Development Command, U. S. Army. The organization is now known as US APRO (U. S. Army Personnel Research Office).

The University of Wisconsin announces the organization of a *Department of Educational Psychology*, to begin with the 1962 Summer Session, June 18. Five areas will be represented: human learning, human development, school psychology,

and remediation (including remedial reading), measurement, and statistics and research design. For further information, write to Julian C. Stanley, Education Building, University of Wisconsin, Madison 6, Wisconsin.

The 1962 Workshop in the Rorschach Technique of Personality Diagnosis and Other Techniques as Used with Children, sponsored by the Claremont Graduate School in Claremont and the Childrens Hospital, Los Angeles, will be held July 8-20, 1962 at Asilomar Conference Grounds, Pacific Grove, California. For details and applications write to Bruno Klopfer, P. O. Box 2971, Carmel, California, before June 1, 1962.

The Educational Testing Service has announced its Second Annual Workshop in Test Construction for Foreign Scholars. Designed to give participants *practical training in measurement*, the six-week program will be from June 25 to August 3, 1962. Additional information may be obtained from: Director of Advisory and Instructional Programs (Workshop in Test Construction), Educational Testing Service, Princeton, New Jersey. Applications must be received in Princeton by April 15.

The seventh annual conference of the American Academy of Psychotherapists will be held on October 6-7, 1962 at the Palmer House in Chicago. The theme of the conference is "How Can the Scientist Help the Psychotherapist." For further information write to: Albert Ellis, Parc Vendome, 333 West 56th Street, New York 19, New York.

The Eastern Colleges Science Conference for 1962 will meet at North Carolina State College, Raleigh, on May 3-5. The major purpose of the Conference is the stimulation of *undergraduate interest in the natural sciences*, including psychology, by providing a forum for the presentation of undergraduate research papers. Further information can be obtained from Mr. Phillip Nanzetta, Executive Committee, Eastern Colleges Science Conference, North Carolina State College, Raleigh.

The University of Michigan will hold its 15th Annual Conference on Aging at the Michigan Union in Ann Arbor June 18-20, 1962. The topic of the conference will be "Aging and the Economy." For further information, write to Wilma Donahue, Division of Gerontology, The University of Michigan, Room 1510 Rackham Building, Ann Arbor, Michigan.

In connection with the dedication of the Carl Campbell Brigham Library, the Educational Testing Service commissioned the preparation of a *biographical sketch* of Dr. Brigham. Copies are available to interested persons upon request to the Educational Testing Service, Princeton, New Jersey.

A set of largely unbound copies of the *Journal of Abnormal and Social Psychology* for the years 1907-1915 are available to any library which would like to have them. Write to Arnold A. Madow, Faribault State School and Hospital, Faribault, Minnesota.

INTERNSHIPS FOR DOCTORAL TRAINING IN CLINICAL PSYCHOLOGY APPROVED BY THE AMERICAN PSYCHOLOGICAL ASSOCIATION

November 1, 1961

THE Education and Training Board has published a list of approved predoctoral internships in clinical psychology. The internships in the listed agencies met minimal standards published in the *American Psychologist*, 1958, 13, 59-60. The last listing was compiled as of November 1, 1959, and was published in the *American Psychologist*, 1961, 16, 25-29. A moratorium was placed on new additions pending further study and analysis in November 1959.

By action of the Council of Representatives at the 1961 Annual Meeting in New York City, the moratorium on evaluation of predoctoral internship centers in clinical psychology was lifted. The Committee on Evaluation is proceeding to carry out the provisions of a revised system. The new listing of the approved internships will be published in the *American Psychologist* as soon as it is ready.

—SHERMAN ROSS

CONVENTION ANNOUNCEMENTS

SEVENTIETH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

GEORGE S. SPEER
Convention Manager

AND

JAMES M. VANDERPLAS
Associate Convention Manager

Time and Place: Thursday, August 30, through Wednesday, September 5, 1962 in St. Louis. St. Louis is on Central Daylight Saving Time. All meetings and all official APA functions including the APA Day Program will be held in the Headquarters Hotel, the Chase Park Plaza.

St. Louis offers a number of attractions, including a world famous zoo, the world's newest planetarium, the fabulous Climatron, the exquisite Jewel Box, the world renowned Shaw's Gardens, and the ever lovely Municipal Opera, "Opera under the stars." During the period of the Convention we may expect temperatures to range from highs in the 90s to lows in the 70s. Nights are generally cooler in the west end, too, than they are downtown.

Special Attraction—Wear Your Badge: For the convenience of our members, a complimentary bus service will be maintained, serving the Chase Park Plaza, Coronado, and Sheraton Jefferson Hotels, on a frequent schedule. Members *must* wear their convention badges to use this complimentary service.

Also, swimming pools at the Chase Park Plaza, Bel Aire, Diplomat, and Ambassador hotels will be available for use by all members of APA who (1) are registered as convention delegates, and (2) present their badges at time of use of pool facilities.

Housing: In cooperation with APA, substantial blocks of rooms have been set aside at guaranteed rates at all of the cooperating hotels. *Members should note, however, that these rates are guaranteed at the rates requested only if the Advanced Registration Form is returned prior to August 1, 1962.* After August 1, every effort will be made to assign rooms at the guaranteed rate, but such assignments cannot be guaranteed, and it is quite unlikely that late requests can be honored. Reservations received *after* August 1 would be assigned at the regular hotel rates, which are considerably higher than the flat rates arranged for our convention. The Advanced Registration Form (which includes space for hotel reservations) appears at the end of these announcements. Members and guests with special housing needs should write directly to the Housing Committee Chairman: Rev. Francis T.

Severin, Department of Psychology, St. Louis University, St. Louis 3, Missouri.

Registration: Members and guests are urged to register in advance to minimize delay upon arrival at the meetings, as well as to ensure guarantee of hotel space at desirable rates. Attention is called to a continuation of the advanced registration procedure introduced last year. Again, this year, the convention badge, with name and institutional affiliation, will be mailed in advance of the convention *to those who preregister.* Advanced registrants will need only to put their local convention address on a coupon and hand this to the convention clerks to complete their registration, thus almost completely avoiding any possible delays.

Complete member and nonmember registration facilities will be maintained in the Chase Club Lounge of the Chase Park Plaza Hotel. Advanced Registration facilities will be maintained in the Coronado and Chase Park Plaza Hotels. There is no fee for APA members, foreign affiliates, members of Psi Chi, members of the Student Journal Group, or *nonmembers who are participants in the official program*; the fee for other nonmembers is \$3.00.

The Registration Desks will be open on Wednesday, August 29, from 2:00 P.M. to 8:00 P.M. During the meetings the Registration Desks will be open from 8:00 A.M. to 5:00 P.M., except for APA Day, Sunday, September 2, and on Wednesday, September 5, on both of which days the desks will close at 12:00 noon.

Directory: A directory of members and guests registered at the convention will be maintained in the Lindell Foyer of the Chase Park Plaza and in the Bar Madrid of the Coronado hotels. New directory listings will be posted twice daily.

Mail and Directory Correction: A message bulletin board and mailbox for leaving messages for members and guests at the convention will be available in the Lindell Foyer of the Chase Park Plaza Hotel. Messages may also be left at the Coronado Hotel for delivery to the Chase Park Plaza. There will also be a Directory Correction Desk in this area. Mem-

bers and guests who notice errors in the directory listing are urged to complete a correction form and to have their listing revised. However, members and guests are cautioned that corrections will be made only in the spelling of the name, and in their local convention address.

Tickets for Luncheons, Dinners, and Other Special Events: It will be necessary to purchase tickets for all scheduled meal functions and other ticketed special events in advance of the function. Tickets will be sold only at the Special Events Desk in the Chase Club Lounge of the Chase Park Plaza Hotel. Luncheon tickets must be purchased before 10:00 A.M. and dinner tickets before 3:00 P.M. on the day for which the event is scheduled.

Additional Luncheon and Dinner Requests: Arrangements for food or beverage functions for special groups, APA divisions, alumni parties, special interest groups, etc. may still be made. After April 15 (see "Call for Papers and Symposia" in the November 1961 *American Psychologist*), requests should be sent to the Associate Convention Manager as soon as possible. Functions scheduled before July 15 will be announced in the *Convention Guide*.

Women's Activities: Arrangements have been made for a Distaff Center in the Palladian Room, Chase Park Plaza Hotel. This center will open only to women from 9:00 A.M. to 5:00 P.M. every day except APA Day. Coffee will be served with the compliments of the Women's Activities Committee.

APA Reception for Psychologists' Wives and Guests: On Monday, September 3, in the Zodiac Room of the Chase Park Plaza Hotel from 4:30 to 6:00 P.M., the Women's Activities Committee will hold a reception in honor of the wives of the APA officers, the directors, and the distinguished guests. Although this reception has been planned primarily for women in psychology, the committee extends a cordial invitation to all who are interested in attending.

APA Dance: The APA Dance will be held on Saturday, September 1, in the Khorassan of the Chase from 9:00 P.M. to 1:00 A.M. There is no admission charge for this event. Provision will be made for the purchase of beverages.

Exhibits: This year a very interesting array of educational, informative, and entertaining exhibits has been planned in the exhibit area. Exhibits will be on the Exhibit Floor of the Chase. Members are urged to visit these unusual displays. An informal lounge will also be set up in this area, and favors and entertainment will be provided.

Convention Lounge: The Ridibunda Lounge, open to all members and guests, will be located in the Tiara Lounge, 26th floor of the Park Plaza section of the Chase Park Plaza Hotel. The lounge and bar facilities will be open at 4:00 P.M. daily. Social hours for many divisions and other groups will be scheduled in the Ridibunda Lounge.

Placement: The Placement Office will be located in the lower level of the Chase, adjacent to the Exhibit area. The Placement Office will be open Wednesday, August 29, from 2:00 P.M. to 8:00 P.M., and daily (except APA Day when the office will be closed) from 9:00 A.M. to 5:00 P.M. Applicants and employers are urged to preregister with the Placement Office. Applicants seeking employment may preregister by completing the Applicant Form which appears at the end of these announcements. Employers having position openings may preregister by completing the Position Description Form which also appears at the end of these announcements. A special publication, the *Convention Placement Bulletin*, which will include all preregistered applicants and position openings, will be available for sale at the Placement Office at \$1.00 per copy.

Convention Treasurer: The Convention Treasurer urges division officers to check their financial commitments with the hotels very closely to avoid confusion with those commitments assumed by the APA Central Office or by the Convention Arrangements Committee. In general, divisions are urged to check with the Convention Manager or the Associate Convention Manager before making any commitments which involve financial responsibility.

Audio-Visual Aids: All persons who are planning to use slides, films, or other audio-visual aids should reread with care Section VI of the "Call for Papers and Symposia" in the November 1961 issue of the *American Psychologist*. Projection will not be provided for those who have not complied with the procedures outlined in the call. **NOTE THAT THIS IS A CHANGE FROM PREVIOUS PROCEDURES.** Hans Schmidt, Washington University, St. Louis 30, Missouri.

Volunteer Workers: At each convention, there is need for volunteer workers to supplement the various committees and to assist in staffing desks, offices, etc. Most of the work during the convention proper (as opposed to the long hours of detailed committee planning) is done by members and students who have volunteered their services. This year's Volunteer Workers Committee would appreciate offers to work or to recruit students who will work. If you will have

some free time during the convention and would like to help, please fill out the Call for Volunteers Form at the end of these announcements and return it to the Chairman of the committee: Norman L. Corah, Psychology Department, Washington University, St. Louis 30, Missouri.

Convention Personnel: A convention of this size requires the efforts of many people for many hours far in advance, during the convention, and for some considerable time later. Each person assumes responsibility for one aspect of the convention; all work together cooperatively to provide you with a pleasant and smoothly operating convention. You may wish to contact one or more of these people before or during the convention.

Convention Manager: George S. Speer, *Illinois Institute of Technology, Chicago*

Associate Convention Manager: James M. Vanderplas, *Washington University*

Convention Program Committee: Marion E. Bunch, *Washington University*

Convention Treasurer: King M. Wientge, *Washington University*

Audio-Visual Aids: Hans Schmidt, *Washington University*

Convention Guidebook: Frederick J. Thumin, *Washington University*

Directory and Mail: Allan G. Barclay, *St. Louis University*

Housing: Francis T. Severin, *St. Louis University*

Information Desk: V. Ralph Buzzotta, *Psychological Associates, Inc.*

Meeting Rooms: Wendell S. Phillips, *St. Louis University*

Membership Survey: Robert L. Lefton, *Psychological Associates, Inc.*

Placement Liaison: Abel G. Ossorio, *St. Louis State Hospital*; and Robert F. Lockman, *APA Central Office, Washington*

Public Relations: Michael Amrine, *APA Central Office, Washington*; and John A. Stern, *Washington University*

Registration: Richard deCharms, *Washington University*; and Daniel S. Lordahl, *Washington University*

Special Materials: Donald Kausler, *St. Louis University*
Special Events: D. Gene Davenport, *St. Louis University*

Volunteer Workers: Norman L. Corah, *Washington University*

Women's Activities: Loretta M. Cass, *Washington University*

Location of Headquarters Rooms: Following is a list of room locations for convention activities and convention personnel in addition to the activity locations listed above in these announcements.

APA Board of Directors: *Lacquer*

APA Central Office: *Swedish*

APA Convention Offices: *Lucas Place*

Distaff Center: *Palladian*

Film Presentations: *Regency*

Physical Facilities (Audio-Visual Aids, Meeting Rooms, Signs and Posters): *Check Room*

Public Relations and Press: *Park*

Volunteer Workers: *Pony Express*

Location of Function Rooms:

CHASE SECTION

Lower Level: Exhibit Hall, Placement

Ground Floor: Khorassan, Chase Club, Pony Express, Check Room, Lucas Place, Lido, Regency

Second Floor: Hunt, Colonial, Park, English, Embassy, Palladian

Ninth Floor: Starlight, Zodiac

PARK PLAZA SECTION

First Floor: Georgian

Mezzanine: Regency, Stockholm, Venetian, Swedish, Silver, Lacquer

26th Floor: Tiara Lounge

27th Floor: Tiara Room

St. Louis, Missouri, August 30–September 5, 1962

Circle One:	Prof.	Name:	First name or initials only	Last name only
	Dr.			
	Mr.			
	Mrs.			
	Miss			

[illegible]

.....
(Street Address)
.....
(City) (Zone) (State)

□ □ □ □ □ □ □

Nonmember REGISTRATION FROM NONMEMBERS MUST BE ACCOMPANIED BY A REGISTRATION FEE OF \$3.00. PLEASE MAKE CHECKS PAYABLE TO: APA CONVENTION AFFAIRS BOARD.

HOTEL INFORMATION

..... I do not want a hotel reservation

Please check carefully the hotel and rate information shown on opposite page. Be sure to complete all of the information requested below:

Type of accommodation desired: Single Double Twin Dormitory

Hotel preference:

Rate requested: \$.....

1.

2.

3.

When more than one person will occupy the room, please give the following information for all occupants:

Name (please print)	Sex	Address	City	State
.....
.....
.....
.....

Reservations will not be held beyond 6:00 P.M. except by request.

NOTE: THESE RATES ARE GUARANTEED AT THE RATE REQUESTED ONLY IF THE REGISTRATION BLANK IS RETURNED PRIOR TO **AUGUST 1, 1962**. AFTER AUGUST 1 EVERY EFFORT WILL BE MADE TO ASSIGN ROOMS AT THESE RATES, BUT SUCH ASSIGNMENT CANNOT BE GUARANTEED.

MEMBERS ARE URGED TO RETURN THIS FORM PROMPTLY, AND IN ANY EVENT PRIOR TO **AUGUST 1**, IN ORDER TO BE SURE OF RECEIVING THE ACCOMMODATIONS DESIRED.

Your hotel reservation will be confirmed, and will be mailed to you with your convention badge. Please be sure the mailing address and other information requested is correct and complete.

Please return this form as early as possible to:

APA Housing Bureau, 911 Locust Street, St. Louis 1, Missouri

(Copies of this form may be obtained from the APA Central Office)

Advanced Registration Forms received after August 1 may not be processed for preregistration

HOTEL AND RATE INFORMATION

SEVENTIETH ANNUAL CONVENTION OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

St. Louis, Missouri, August 30-September 5, 1962

HEADQUARTERS HOTEL: Chase Park Plaza

PARTICIPATING HOTELS AND MOTELS:

West Central Area: Ambassador, Bel Air, Roosevelt, Claridge, Coronado, Congress, Diplomat

Downtown Area (remote): Sheraton-Jefferson, Statler-Hilton, Pick-Mark Twain

ALL SUBSTANTIVE PROGRAM EVENTS WILL BE HELD IN THE CHASE PARK PLAZA HOTEL.
RELATED EVENTS AND MEETINGS OF ASSOCIATED GROUPS WILL BE HELD PRIMARILY
IN THE WEST CENTRAL AREA HOTELS.

Hotel or Motel	Rate and Type of Accommodation													
	\$5	\$6	\$7	\$8	\$9	\$10	\$11	\$12	\$13	\$14	\$15	\$16	\$17	\$18
Ambassador				S	S	S D	D	D T	D T	T	T			
*Bel Air Motel						S S	S	S	S D	S D T	D T	D T	D T	T
*Chase Park Plaza					S	S	S	S D	D	D T	D T	D T	T	T
Claridge	S	S	S	S D	D T	D T	D	T		T				
*Congress			S S	S	T				T					
Coronado				S	S	S	S D	D		D T	D T	T	T	
*Diplomat Motel					S	S	S	S D	D	D	T	T	T	
*Pick-Mark Twain				S	S		D	D T	T					
*Sheraton-Jefferson				S		S	S	D T		S D T	T			
*Statler-Hilton				S	S	S	D	S D		D T	D T		T	

S, single; D, double; T, twin.

* These hotels and motels completely airconditioned, others are partially airconditioned.

DORMITORY ROOMS ARE AVAILABLE IN THE ROOSEVELT HOTEL, 2, 3, and 4 PERSONS
IN A ROOM AT \$3.50 PER PERSON.

SUITES ARE AVAILABLE IN ALL HOTELS AND MOTELS AT RATES FROM \$14 to \$75 PER
DAY. PLEASE WRITE DIRECTLY TO THE HOUSING BUREAU FOR THIS TYPE OF
ACCOMMODATION.

Publications of the AMERICAN PSYCHOLOGICAL ASSOCIATION

Subscription Rates for 1962

American Psychologist. Official papers of the Association and articles on psychology. Monthly. First issue appears in January. The 1962 volume is Vol. 17. Subscription: \$10.00 (Foreign \$10.50). Single copy \$1.00.

Contemporary Psychology. Critical reviews of books, films, and research material in the field of psychology. Monthly. First issue appears in January. The 1962 volume is Vol. 7. Subscription: \$10.00 (Foreign \$10.50). Single copy \$1.00.

Journal of Abnormal and Social Psychology. Original contributions in the field of abnormal and social psychology, and case reports. Monthly, two volumes per year. There are 6 issues in each volume. First issue appears in January. The 1962 volumes are Vols. 64 and 65. Subscription: \$20.00 for 2 vols. (Foreign \$20.50). Single copy \$2.00.

Journal of Applied Psychology. Applications of psychology to business and industry. Bimonthly. The issues appear in February, April, June, August, October, and December. The 1962 volume is Vol. 46. Subscription: \$10.00 (Foreign \$10.50). Single copy \$2.00.

Journal of Comparative and Physiological Psychology. Original contributions in the field of comparative and physiological psychology. Bimonthly. The issues appear in February, April, June, August, October, and December. The 1962 volume is Vol. 55. Subscription: \$10.00 (Foreign \$10.50). Single copy \$2.00.

Journal of Consulting Psychology. Research in clinical psychology: psychological diagnosis, psychotherapy, personality, psychopathology. Bimonthly. The issues appear in February, April, June, August, October, and December. The 1962 volume is Vol. 26. Subscription: \$10.00 (Foreign \$10.50). Single copy \$2.00.

Journal of Educational Psychology. Studies of learning and teaching: measurement of psychological development, psychology of school subjects, methods of instruction, school adjustment. Bimonthly. The issues appear in February, April, June, August, October, and December. The 1962 volume is Vol. 53. Subscription: \$10.00 (Foreign \$10.50). Single copy \$2.00.

Journal of Experimental Psychology. Original contributions of an experimental character. Monthly, two volumes per year. There are six issues in each volume. First issue appears in January. The 1962 volumes are Vols. 63 and 64. Subscription: \$20.00 for 2 vols. (Foreign \$20.50). Single copy \$2.00.

Psychological Abstracts. Noncritical abstracts of the world's literature in psychology and related subjects. Bimonthly. The issues appear in February, April, June, August, October, and December. The 1962 volume is Vol. 36. Subscription: \$20.00 (Foreign \$20.50). Single copy \$4.00.

Psychological Bulletin. Evaluative reviews of research literature, discussions of research methodology in psychology. Bimonthly. The issues appear in January, March, May, July, September, and November. The 1962 volume is Vol. 59. Subscription: \$10.00 (Foreign \$10.50). Single copy \$2.00.

Psychological Monographs: General and Applied. Longer researches and laboratory studies which appear as units. Published at irregular intervals during the calendar year — between 15 and 20 issues per year. The 1962 volume is Vol. 76. Subscription: \$10.00 (Foreign \$10.50). Single copy \$1.00 to \$3.00 according to size.

Psychological Review. Original contributions of a theoretical nature. Bimonthly. The issues appear in January, March, May, July, September, and November. The 1962 volume is Vol. 69. Subscription: \$10.00 (Foreign \$10.50). Single copy \$2.00.

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APPLICANT FORM

(Typewrite or print in pencil) **APA CONVENTION PLACEMENT OFFICE** **Applicant Number**.....

Name: **Sex:**

Convention address: **Phone:**

Permanent address:

APA membership: Yes No

Academic Training: Degree	University	Year
Describe pertinent work history:		
Number of papers presented or published:		

Type of position desired:

Expected salary (optional): **Preferred location:**

Give a tentative schedule of the time you plan to be available in the Convention Placement Office for interviews:

Aug. 30		Aug. 31		Sept. 1		Sept. 3		Sept. 4		Sept. 5	
A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.

Under which field of specialization do you wish your application to be filed? Check no more than two boxes:

	Clinical
	Counseling
	Educational School

	Developmental
	Experimental
	General
	Physiological

	Human Engineering
	Industrial
	Personality
	Social

(Return completed form to: Convention Placement; 1333 Sixteenth Street, N.W.; Washington 6, D. C.
Copies of this form are available from this address.)

**APPLICANT FORMS RECEIVED AFTER AUGUST 1 MAY NOT BE
 PROCESSED FOR PREREGISTRATION.**

PSYCHOLOGY AND REHABILITATION

BEATRICE A. WRIGHT, Ph.D.

Editor

Based on the proceedings of an Institute on the roles of psychology and psychologists in rehabilitation, sponsored by the American Psychological Association and supported by a grant from the Office of Vocational Rehabilitation, Department of Health, Education and Welfare.

The volume examines the principles and assumptions underlying the modern philosophy of rehabilitation, paying special attention to the roles and functions of psychologists as members of an interdisciplinary team. Problems of training are explored in detail and in the context of the changing nature of the field. Areas of needed research are highlighted.

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POSITION DESCRIPTION FORM

(Typewrite or print in pencil) APA CONVENTION PLACEMENT OFFICE Position Number.....

I. Name and Address of Employing Organization:

II. Position Title (include location if different from I.):

III. How many openings does this form cover:

IV. Applicant Requirements:

A. Degree: 1. PhD (EdD) 2. PhD preferred, will consider Master's
3. Master's 4. Other.....

B. Experience:

C. Other:

Sex: Male Female Either

V. Position requirements:

A. Duties:

B. Starting Salary (depending on qualifications): Minimum Maximum

C. Position Begins: Immediately or, give date

VI. A. If you wish space to interview applicants, fill in the name of the interviewer and check your tentative interviewing schedule:

Aug. 30		Aug. 31		Sept. 1		Sept. 3		Sept. 4		Sept. 5	
A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.	A.M.	P.M.

B. If you have no plans for interviewing, please indicate the person to whom inquiries are to be sent:

VII. Check one category for listing your position:

Academic Clinical and Counseling Industrial and Research

(Return completed form to: Convention Placement; 1333 Sixteenth Street, N.W.; Washington 6, D. C. Copies of this form are available from this address.)

POSITION DESCRIPTION FORMS RECEIVED AFTER AUGUST 1 MAY NOT
BE PROCESSED FOR PREREGISTRATION.

ANNOUNCEMENT

Seminar on the Fundamentals of Rorschach Testing in the Appraisal of Personnel

- A Study of the Administration, Scoring, and Interpretation of the Rorschach and Its Application to Selection, Counseling, and the Detection of Adjustment Problems.
- For Persons with Psychological Training Involved in Management-Development Programs, Selection, Appraisal, and Counseling, Desiring to Gain Proficiency in This Technique.
- Conference Leader: Dr. Marguerite Hertz, Clinical Professor of Psychology, Western Reserve University.

Dates: April 16, 17, 18, 19, 20, 1962. **Place:** Hitchcock Hall, Western Reserve University

Fee: \$200 including all mimeographed materials

Information on Request

Dr. Jay L. Otis

Psychological Research Services
Western Reserve University
Cleveland 6, Ohio

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American Psychological Association
1333 Sixteenth Street, N. W.
Washington 6, D. C.

CALL FOR VOLUNTEERS

ANNUAL CONVENTION: AMERICAN PSYCHOLOGICAL ASSOCIATION

St. Louis, August 30–September 5, 1962

It takes people—preferably people with psychological background—to run a convention. Would you be willing to help? If you are planning to come to the convention and can devote at least two periods (mornings, afternoons, evenings) to one of the many jobs that must be filled to run a smooth convention, would you please indicate the periods you would be available. If you are not an APA member or in the Student Journal Group or Psi Chi, the \$3.00 Registration Fee for nonmembers will be waived, as a small token of our appreciation. At the convention you will receive a program guide, and your lapel badge will admit you to all sessions.

1. I volunteer for a maximum of _____ periods from the ones indicated below.

2. Place the number 1 in each of your two (or more) first-choice times, the number 2 in each of your second-choice times, and the number 3 in each third-choice time. If you are concerned about avoiding time conflicts with programs of interest, consult the convention schedule in the November *American Psychologist* or the official program in the July *American Psychologist*.

	Wed. Aug. 29 Pre- convention	Thurs. Aug. 30	Fri. Aug. 31	Sat. Sept. 1	Sun. Sept. 2	Mon. Sept. 3 Labor Day	Tues. Sept. 4	Wed. Sept. 5
8:45–12:45								
12:45– 4:45								
4:45– 8:15								

Note: There *will* be mealtime breaks for those who serve successive periods on the same day.

3. If you volunteered for a morning session, could you begin at 8:15 A.M. if necessary? Yes ☐ No ☐

4. Check the services you offer:

_____ General (clerical _____, filing _____, information desk _____, sign painting _____, etc.)

_____ Typing (less than 50 words per min. _____, 50 words per min. or more _____)

Name: _____ Phone No.: _____ Date: _____

Mail Address: _____ City: _____ Zone: _____ State: _____
(on August 20)

School (if a student now) _____ VA trainee _____

VA installation _____

Position (if *not* a student): _____ Organization: _____

5. Please detach and mail above to: Norman L. Corah, Dept. of Psychology, Washington University, St. Louis 30, Missouri.

6. You will hear from us by August 20th notifying you of your assignment. If you do not, please come to the Volunteer Workers Desk on your arrival at the Convention (Lucas Room, Chase). *If a change occurs in your address or availability, please write to Corah (at the above address). Fill out, detach, and save this duplicate slip.*

	Wed. Aug. 29 Pre- convention	Thurs. Aug. 30	Fri. Aug. 31	Sat. Sept. 1	Sun. Sept. 2	Mon. Sept. 3 Labor Day	Tues. Sept. 4	Wed. Sept. 5
8:45–12:45								
12:45– 4:45								
4:45– 8:15								

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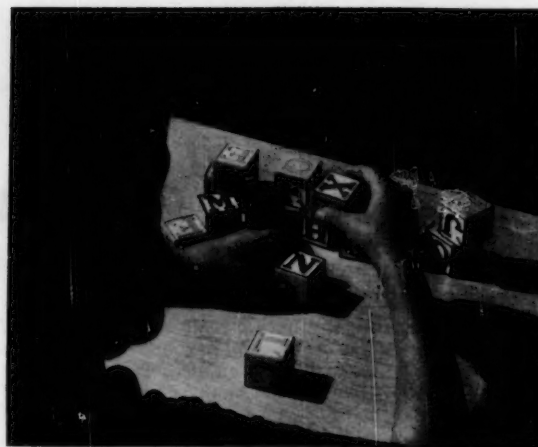
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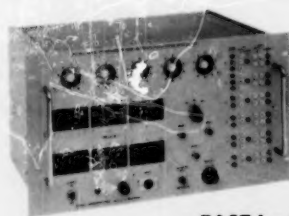
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- time interval (eg., reaction time) measurement 1/10 ms to 1000 sec (depending upon choice of clock)
- generation of time intervals (up to six independent values) periodically or on a single-frame basis
- On/Off (and other) waveform synthesis (6 channels): the On/Off waveforms can be algebraically summed and/or integrated by analog amplifiers to generate staircase, ramp, sawtooth and other functions.



FAST Instrumentation Components
Model 5656 Programmed Time Interval/Waveform Generator . . . \$1390
Clock option for Model 5656 \$100



Model A102 power supply (powers 1, 2, or 3 A101 light modules independently), with provision for remote control of light intensity . . . \$295



Model A101 light modules \$85 each



Iconix Tachistoscope

Model 531 (includes 3 A101 light modules)—an electronic tachistoscope designed specifically for the psychologist. Though not as advanced as the FAST system, performs most of the FAST functions, and is easy to operate \$1975

Write for technical data sheets—

iconix
incorporated

945 Industrial Avenue

Palo Alto, California

